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CONTRIBUTIONS TO ANTHROPOLOGY:

ECOLOGICAL ESSAYS

Proceedings of the Conference on Cultural Ecology
Ottawa, August 3–6, 1966

Edited by David Damas

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Foreword

Recent years have seen a resurgence of interest in cultural ecology with a view to the development of more precise formulations and rigorous methodology. The present volume of Contributions to Anthropology is the result of the Conference on Cultural Ecology, held at the National Museum of Canada, August 3–6, 1967. The Conference was planned and organized by David Damas, Arctic Ethnologist of the Museum.

Fourteen anthropologists whose recent field-work and theoretical interests were focused on the exploration of relationships between culture, society, and environment met for a four-day Conference. Papers were given on specific societies in North America, Oceania, and Africa; others dealt with cross cultural analysis. These papers were followed by discussion, criticism, and evaluation of the present state of research. In this volume formal papers are accompanied by the discussions, which are, in themselves, of considerable value.

The Conference was the fourth sponsored by the Ethnology Division of the Museum. The first, in 1963, was concerned with Museums and anthropological research. The second, in 1964, was devoted entirely to Algonkian linguistics, the results of which are available in published form (Contributions to Anthropology: Algonquian Linguistics I, Bulletin 214, 1967). The third conference, held in 1965, dealt with band organization (Contributions to Anthropology: Band Societies, Bulletin 228, 1969).

These conferences have stimulated related research, and it is hoped that the present volume will perform a similar function.

J. G. E. Smith
Acting Chief Ethnologist

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Résumé

L'introduction au présent volume d'essais écologiques offre un exposé historique des travaux de recherche effectués en écologie culturelle et une appréciation de l'importance de la conférence sur l'écologie en fonction d'études antérieures. Elle examine aussi les mérites et les limites des méthodes d'analyse écologique.

La première partie du volume comprend une série de mémoires sur l'écologie culturelle appliquée à un ensemble de données relatives à des sociétés particulières. On y présente, entre autres, des études ethnographiques et ethnohistoriques de peuples d'Afrique et de Nouvelle-Guinée et des données archéologiques et ethnographiques sur les Nord-Américains. Les études traitent pour la plupart d'une seule société mais, dans plusieurs cas, l'application de méthodes de comparaison sert à établir des hypothèses ou des généralisations. L'intérêt est concentré sur deux points: l'organisation sociale, et l'interaction des facteurs historiques et écologiques. D'autres études démontrent les procédés méthodologiques d'analyse écologique en insistant particulièrement sur la quantification.

Ces points d'intérêt et d'autres, d'ordre théorique, sont abordés dans la seconde partie, qui comprend une série de mémoires et de comptes rendus de délibérations sur le sujet. Murdock fait l'examen de 322 sociétés pour établir des relations entre leurs modes de subsistance (chasse, pêche, élevage, agriculture) et d'établissement (nomade, semi-nomade, semi-sédentaire, sédentaire).

Helm aborde l'étude des relations qui existent entre le mode d'établissement (le passage de nomade à sédentaire) et le mode de groupement (plus précisément, à un endroit particulier ou par familles) et Rogers oriente les délibérations sur l'étude des facteurs historiques dans la recherche écologique, entre autres en fonction des caractéristiques de l'organisation sociale.

La méthodologie de l'analyse écologique fait l'objet d'une étude plus approfondie et, à la fin du volume, Rappaport donne un exposé sommaire des principes de méthodologie.

INTRODUCTION:

THE STUDY OF CULTURAL ECOLOGY AND THE ECOLOGY CONFERENCE

BY

DAVID DAMAS

One of the first sound correlations established between features of society and environment came from Boas (1888: 417), who related conditions of sea-ice with Eskimo settlement.[1] On the one hand, here was a clear association between the formation of sea-ice and the habits of the ringed seal, and, on the other hand, the exploitation of the seal and the distribution of Eskimo population. Despite his espousal of this and other associations between man and his habitat, Boas (1896: 906) criticized 19th century environmentalists and concluded that "environment has a certain limited effect upon the culture of man, but I do not see how the view that it is the primary moulder of culture can be supported by any facts." He found that historical factors, especially diffusion, provided greater explanatory utility in understanding the distribution of cultural traits.

Later, interest in culture area studies, though basically involved in a spatial reduction of the temporal phenomena of diffusion, reached the extreme in Wissler (1926) in relating culture to environment. Wissler (1926: 182) saw the "one great problem in anthropology" to be the study of distributions, and the environment to be the mechanism behind the distribution. His position is summarized as follows:

> . . . the environment is in some way a determiner, and a principle, or law, may be formulated as, when two sections of a continent differ in climate, florae and faunae, or in their ecological complexes, the culture of the tribal groups in one section will differ from that in the other. (Wissler 1926: 214)

Dixon's (1928) critique of Wissler's arguments included disavowal of what he saw as the latter's too close identification of cultural distributions with features of the habitat, and he (Dixon 1928: 12–13) commented that "it is in its beginnings . . . that culture leans most heavily upon environment" and "in the main environment is permissive, not mandatory."

The culture area concept was developed further by Kroeber (1939), who gave detailed treatment of both natural and cultural areas. However, his assessment of the relationship between these two entities was qualified: "The interaction of culture and environment becomes exceedingly complex when followed out. And this complexity makes generalization unprofitable on the whole" (Kroeber 1939: 205). Kroeber (1939: 1) also stressed that "the immediate causes of cultural phenomena are other cultural phenomena."

In the years just prior to Kroeber's important work on cultural and natural areas, interest in the relationships between culture, society, and habitat continued on both sides of the Atlantic. Forde's (1934) well-known study examined several societies from various parts of the world in terms of their relationships to their economies and habitats, but he found that no simple correlations or generalizations emerged. He viewed physical conditions as entering into cultural development "not as determinants but as one category of the raw material of cultural elaboration" (Forde 1934: 464). He criticized both the doctrines of geographical and economic determinism and added "indeed, the economy may owe as much to the social and ritual pattern as does the character of society to the economy" (Forde 1934: 465). Forde also stressed the role of diffusion operating in the areas of both cultural and social features.

Forde's rather negative view of the effects of environment or economy on society or culture was contrasted during the same period by Julian Steward's revival of interest in environmental–cultural relationships. While Steward (1938: 260) stressed that his approach was "neither extreme environmental determinism nor economic determinism," he emphasized factors which were basically technological–economic–environmental in nature when he reviewed factors which he found to be important in the development of societal types. He paid special attention to hunting and gathering groups, underlining ecological factors which he thought were involved in the development of patrilineal, composite, and matrilineal bands (Steward 1936, 1937, 1938). Steward (1937) hypothesized the evolution of bands to multi-band villages and to the appearance of clans in the Southwest. While he has stated (Steward 1938: 262) that the relationship between society and subsistence "becomes more difficult to ascertain in complex societies," he attributes social changes in complex societies principally to economic causes.

Steward's contributions to ethnology in the 1930's are important in several respects. His predecessors had overwhelmingly supported diffusion (though not necessarily the simplistic diffusionism of the culture area concept as developed by Wissler or that of the Kulturkreise school) as the chief factor in the occurrence or distribution of cultural traits, while rejecting many or most of the claims that had been made for adaptation of culture to environment. Steward sought to systematize investigations of cultural–environmental relationships. The study of these relationships began to emerge on a new level of sophistication with Steward's work. Perhaps the most significant of Steward's contributions at this time was his recognition of the importance of technology and the techniques of exploitation, a consideration which stressed an important ingredient in the environmental–cultural–social nexus. The expectations of gross environmental–cultural or social relationships that had characterized much of the preceding studies had principally failed to appreciate the importance of this intervening variable.

Steward also directed much of his interest toward understanding development or evolution of social features, paralleling the interest in social anthropology that was blossoming in the United States at the same time through the influence of Radcliffe-Brown (Redfield 1955).

Also helping to point the way to an expansion of social anthropology to include factors of adaptation to the environment was Eggan's (1937) contemporaneous study of the Plains Indians, which explained the convergence of immigrant societies on the plains in terms of possession of common habitat and economy rather than in terms of diffusion.

Concern with culture and personality was another interest in American anthropology of the 1930's and later. Kardiner and Linton (1939) tried to understand personality and projective systems in terms of adaptation to the environment. Areal personality types have been hypothesized for the Plains

(Benedict 1934; Devereux 1951) and the Canadian Subarctic (Honigmann 1954: 334–5). In the latter case, broad associations between environmental or ecological factors and personality have been drawn (low subsistence-conditions–sparse populations–self-sufficiency–inhibition of strong emotions, etc.), but more persuasive are the arguments that the areal distribution of personality traits is spurious in ecological terms and represents evidence for internal diffusion of customary values and child-rearing practices and resistance to diffusion of such values and practices from outside the area in question (Honigmann 1954: 336). However, on the whole, the blending of psychological approaches with a consideration of environmental factors has not met with much enthusiasm in recent years.

By contrast, the problem of relationships between environment and social structure has excited the attention of a number of anthropologists. Murdock's (1949) theory of social evolution rests on the factors which influence rules of residence in societies. These are principally division of labour and other economic considerations (Murdock 1949: 202ff.). Murdock (1949: 196) also discounted the role of diffusion in the distribution of social traits.

Open endorsement of Steward's ecological approach to Southwestern societies came in Eggan's (1950: 320) study of the western Pueblos, which, though basically concerned with a social anthropological analysis of several closely related societies, dealt with historical and environmental factors as well.

With the publication of Steward's *Theory of Culture Change* in 1955, the concept of "cultural ecology"[2] was defined and developed. Steward (1955: 36) says of this approach, "It differs from the relativistic and neo-evolutionist conceptions of culture history in that it introduces the local environment as the extra cultural factor in the fruitless assumption that culture comes from culture."

His earlier implicit distrust of diffusion and its relationship to social and economic features is made explicit at this time: "cultural diffusion, of course, always operates, but in view of the seeming importance of ecological adaptations its role in explaining culture has been greatly overestimated" (Steward 1955: 42).

Though based, in part, on his earlier work, the emergence of the label "cultural ecology" with related concepts of the "culture core" and "levels of sociocultural integration," crystallized in *Theory of Culture Change* and stirred the study of relationships of environment with culture and society to a new level of interest. It is principally since the publication of that work that cultural ecology has come of age as a subdiscipline of social anthropology and of archaeology.

Leslie White (1959) endorsed a more completely economic or technological deterministic philosophy than any other modern-period anthropologist. He sought to demonstrate intrinsic relationships between what he termed "technological," "sociological," and "ideological" levels, and he saw these three levels as a layer-cake formation with technology affecting society and society affecting ideology. Helm (1962: 638–9) has criticized White and his followers for their lack of empiricism, but Sahlins and Service (1960) offered a revised version of Whitean philosophy which should be mentioned. To these authors, "cultural ecology" must embrace the relationships between cultures, "the superorganic setting," as well as the natural features of habitat, just as "ecology in biological studies includes the organic environment, competing species, as well as the inorganic" (Sahlins and Service 1960: 49–50). With the introduction of the concept of the "superorganic environment," ecological studies become an over-arcing category of studies which must encompass historical, cultural, social, and economic factors.

Cultural ecology has recently become an important frame of organization for ethnological field research and for the reports based on field-work. Evans-Pritchard's (1940) classic but qualified appraisal of relationships between environment, economy, and society of the Nuer has been superseded by more recent studies, which conceive of social and cultural systems as being to a large extent products of ecological factors (Sahlins 1958; Leach 1961; Geertz 1963).

Interest in ecological factors has increased in the past few years with archaeological studies (Clark 1952; Braidwood 1953; Willey 1956), as well as ethnological or social anthropological concerns, being affected by the current blossoming of interest in ecology. A number of symposia have been organized (Woodbury *et al.* 1961; Baker *et al.* 1962; Goldschmidt 1965), which crossed the subdivisions of anthropology, and a great proliferation of independent articles has appeared in major professional journals. The brevity of this presentation permits the mention of only a few of these (Meggers 1954; Naroll and D'Andrade 1963; Freilich 1963; Netting 1965; Mencher 1966; Rappaport 1967).

In summary, the history of interest in environmental factors in anthropology has been characterized first by the rather naïve associations of 19th century scholars, followed by a rejection of these associations in favour of historical explanations. A combination and culmination of historical and ecological explanations were reached in Wissler's conception of the culture area. The extremity of Wissler's position was rejected, and diffusionary explanations alone were dominant in ethnology until well into the 1930's. Subsequently, historical explanations have receded into the background as the methodology of cultural ecology has developed and continued to grow. Meanwhile a shift has been made from a comprehensive interest in cultural–environmental associations to specific interest in the adaptation of social systems (especially in ethnology, though to some extent in archaeology as well).

With this brief review of ecological interests in social and cultural anthropology[3] as background, it may be permissible for me to indulge in reference to facts of personal history in order to specify my motivations for calling together this conference on cultural ecology.

During the late 1950's, Julian Steward's *Theory of Culture Change* excited considerable interest among graduate students at Chicago. We found in that book a guidepost to lead us from what we had come to regard as the futile circularity of most social structural studies, studies which were our chief texts in social anthropology. With the further stimulus of Fred Eggan's lectures on his own views of ecology and his endorsement of Steward's and, to some of us at least, the influence of Marshall Sahlins' *Social Stratification in Polynesia*, several of us left for the field in 1960 expecting to find much in our societies that would be explainable in ecological terms. Indeed, several of my fellow students (Nicholas 1962; Netting 1963; Yengoyan 1964) produced polished social anthropological dissertations which were strongly anchored to ecological bases.

In my own field-work at Iglulik (1960–61) I was more concerned with an integrated description of the current and traditional social systems than with an exploration of ecological factors in their development. I felt handicapped in seeking ecological explanations of social phenomena without being able to adequately expand the area of comparison. While certain features of Iglulingmiut society seemed to relate strongly to the economy and habitat, I felt that there were many questions raised that could not be answered without comparing studies of other Eskimo groups within an ecological frame.

A preliminary dissatisfaction with the explanatory value of ecological approaches arose from an examination of the distribution of East-Central Eskimo kinship terminologies (Damas 1963: 206–11), but further field-work gave me reason to accept a qualified role for ecological factors in the Central Eskimo area. Indeed, I found enough of these factors to consider myself committed to continue seeking ecological correlations. At the same time the significant number of anomalies in ecological explanations for social features led me to look for historical factors that might account for the anomalies, and, indeed, the study of cultural ecology became for me a search for the origins of social features, a search which had to be implemented by historical considerations.

At the same time I had had misgivings about having to abandon to the "shreds and patches" of historicalism the functionalism that cultural-ecological approaches imply. I continued to be haunted by the apparent reasonableness of many ecological studies which had been more successful than mine in demonstrating a large number of correlations between habitat or economy and society and culture. I had the unhappy feeling that my approach to ecology had missed something, that it had been too superficial and lacking in precision.

A second source of personal concern with cultural ecology studies came from the National Museum of Canada Conference on Band Organization (August 30 to September 2, 1965). While ecological explanations were secondary to our attempts to sharpen our definitions of bands, to construct typology, and to deal with other related problems, we found that we were clearly pointing toward economic factors as underlying band organization, its characteristics, and its variations. A number of ecological questions were raised at that conference; few were carried far in discussion.

With these private considerations in mind I wrote to a number of anthropologists who had expressed, either verbally or in writing, their involvement in cultural ecology. It became apparent in correspondence that the ecological interests of these scholars were quite varied, and that my personal concern with the interplay of ecological and historical factors was only one of these. With a consequently broadened scope of program in mind, I proposed to the director, Dr. R. G. Glover, a conference on cultural ecology as the fourth in a series of National Museum of Canada conferences. Support was obtained through the approval of Treasury Board, and eventually the following panel of anthropologists met at the National Museum of Canada in Ottawa, August 3 to 6, 1966:

> M. G. Bicchieri, Beloit College; W. Peter Carstens, University of Toronto; David Damas, National Museum of Canada; June Helm, University of Iowa; Richard B. Lee, Harvard University; George Peter Murdock,

University of Pittsburgh; Paul H. Nesbitt, Aerospace Studies Institute; Robert McC. Netting, University of Pennsylvania; Roy A. Rappaport, University of Michigan; Edward S. Rogers, Royal Ontario Museum; David Sanger, National Museum of Canada; James G. E. Smith, National Museum of Canada; Ian Whittaker, York University; Howard D. Winters, University of Virginia.

The present volume contains most of the papers that were read at the conference with the addition of the closing statement by Rappaport, which has been prepared subsequent to the meeting at the editor's request. A number of the papers have been expanded or revised. To some extent, these changes were affected by the discussions which followed each paper. In no case, however, were major conclusions altered from those given at the conference.[4]

In Part I, a series of papers devoted to specific societies depict ecological approaches and problems based on field data. I have sought a worldwide representation in inviting panel members to read these papers. African and New Guinea ethnographic data are presented, as well as North American ethnographic and archaeological material.

Sanger discusses the effects of physiographic changes on economy in the plateau area of the northwest. These changes, together with the operation of historical–diffusionary factors, brought about the re-emergence of a salmon-based economy along with a number of concomitant social and cultural changes.

Rogers argues against "strictly environmental explanations" accounting for variations in Cree and Ojibwa settlement patterns and social organization and enlists witchcraft as the explanatory factor.

In a controlled comparative approach to three Eskimo societies, I delineate social features which appear to relate to adaptive uniformities and variations, while at the same time I denote exceptions to such correlations and suggest historical processes that may account for distributions which are anomalous to ecological explanations.

A virtual laboratory setting for resolving the problem of separating economic and social motives for fusion and fission of hunting–gathering groups is provided in Bicchieri's analysis of the BaMbuti. Permissiveness of habitat during the honey-gathering season allows spatial arrangements of personnel of the sort not exploited during the rest of the year by both archers and net hunters, though the settlement pattern is itself contrasted in the two groups at that time.

Lee demonstrates the utility of an input–output model in ecological studies in his careful study of !Kung Bushman energy requirements and character of production, quantity, and quality of food.

In applying an ethnohistorical approach to Hottentot materials, Carstens indicates that, despite apparent economic advantage in more scattered settlement, religious and defence factors tended to draw settlement together during two main phases of Hottentot history.

Netting argues for the functional interrelationship of population density, farming technique, land availability, household organization, and land tenure in the two Nigerian groups of Igbo and Kofyar. The triggering mechanisms for diverging but concomitant changes in this series of features are seen as being changes in population densities and the relationships of population to arable land.

Rappaport indicates how land is relocated by adjacent New Guinea populations. The redistribution is brought about by the pressures of populations. Both redistribution and ritual ingredients of war are related ultimately to 'ecological competence.'

Part II consists of topical approaches to cultural ecology. The concepts of the *exploitative pattern* (Helm 1962)—the habitat and the tools and techniques for exploiting it—of the *settlement pattern* (Chang 1962)—the spatial and temporal aspects of society—and of the *community pattern* (Chang 1962)—the organizational aspects of society—supply the bases for most of the discussions.

Murdock introduces the discussions on relationships between exploitative and settlement patterns with a survey based on the *Ethnographic Atlas*. The survey indicates that fishing as well as agricultural peoples showed a high tendency toward sedentary settlement, while hunters and pastoralists (and some fishers) were concentrated heavily in nomadic and seminomadic categories. Settlement size related more closely to an agriculture versus hunting–fishing–gathering dichotomy.

The relationship between settlement and community patterns is treated first by Helm, who questions that a straight-line cause and effect progression can be hypothesized from exploitation to settlement to community and finds that "idealogical and social directives can and do affect settlement and exploitation." In discussion, the problem of deriving insights into community pattern from the archaeological record is also dealt with. An attempt is made to correlate settlement and community types (specifically defined in terms of lineality or locality) with points on the nomadic-to-sedentary continuum. Factors of warfare and population structure were seen as possible determinants of community pattern features without necessarily being dependent upon settlement-pattern features.

Questions of historical factors which had been suggested in some of the papers in Part I are raised again by Rogers, who outlines examples illustrating the factors that operate in creating anomalies of ecological explanations. In discussion, the consensus appeared to be that ecological approaches would not be expected to provide complete correlations between society and exploitation and environment. Division of opinion arose over whether historical approaches or the search for origins would have appeal or would be productive.

The section "Methodology of Ecological Analysis" had not originally been planned as a special discussion topic, but because this topic appeared and reappeared during the discussions of the four days of the conference, statements that occurred at various stages in the record of the proceedings have been combined into a separate section. Questions of defining the ecosystem, the ecological population, and the isolation of variables to establish criteria for ecological study are all considered. Finally, the problem of the concept of culture with relation to ecology is posed.

The volume is concluded by a statement of methodological principles by Rappaport, and a further exposition of the position that ecological anthropology can be made "more operational by placing it within the framework of general ecology," a process which he believes will "enhance rather than obscure our understanding of culture."

The papers and discussions are here for the reader to interpret as he will, but a few personal observations may be in order.

The broad range of approaches used in this series indicates something of the variety of applications of the ecological point of view that is being followed in anthropology today. Since, at the same time, all but one or two of the papers were specifically concerned with social features, and since a number of them dealt with historical factors, the two main ecological interests of this writer were served.

Rogers' (*see* below) warning that "explanations in terms of environmental conditions and technological levels are again heading towards an extreme position" should, I think, be heeded, as indeed there appears to have been such a tendency in much of the recent literature on cultural ecology. There did not seem to be any environmental determinists in our number at the conference. Cultural and social factors were generally recognized to intrude on all levels in our attempts to relate society and culture to economy or habitat. However, while there was general agreement that ecological correlations with social or cultural features would not be complete, it became apparent, when actual cases were considered, that our expectations of the range of applicability of ecological analysis did vary rather significantly among us.

The papers and discussions of this volume suggest to me that several major emphases are crystallizing in current interest in cultural ecology.

One of these is the methodological concern with a refinement of technique in the ecological examination of single societies. Searches for understanding human and animal physiology, caloric and other nutritional needs, carrying capacity of land, botany, and climatology comprise important parts of aims in such studies. The superficiality of many approaches to ecology can be highlighted by the results of these sophisticated studies, but they can be applied only to current, viable societies, and appreciation of their level of information is sobering to such workers as myself who have had to rely on reconstruction for much of their information.

While such studies will add much to our body of detailed ethnography, a warning has been raised (*see* Helm in Discussion on Historical Factors) that it may be necessary to reform our traditional social and cultural categories to find meaningful articulations with ecological factors. The problem then raised is: Will this re-examination of categories and their restructuring according to ecologically relevant criteria tend to isolate ecological studies from the chief concerns of social and cultural anthropology, or will it serve to clarify them (as Rappaport suggests)? Much more work will have to be carried out before we can adequately assess such possible implications.

Another methodological concern is that of comparative studies. Lee (Discussion on Historical Factors) has noted that the validity of comparisons rests on the adequacy of the data being compared. Obviously, such detailed quantitative studies as given by Lee and Rappaport will contribute to the meaningfulness of comparisons. Several levels of comparison, ranging from Murdock's use of a wide segment of *Ethnographic Atlas* material to Netting's comparison of Igbo and Kofyar to my study of three contiguous Central Eskimo groups, have been used in this symposium. This concern with comparison has played a prominent part in recent anthropological writings. Eggan (1954: 747) has pointed out the fruitfulness of "controlled comparison":

> My own preference is for the utilization of the comparative method on a smaller scale and with as much control over the frame of comparison as it is possible to secure. It has seemed natural to utilize regions of relatively homogeneous culture or to work within social or cultural types, and to further control the ecology and the historical factors so far as it is possible to do so.

Both types of comparison suggested by Eggan are considered by Steward (1955: 88), who outlines the following accompanying concepts:

> . . . it is proposed that the term *uniformities* be understood to refer to the similarities of form and content which characterize a single area or co-tradition and that *regularities* be understood to mean similarities which recur cross-culturally in historically separate areas or traditions.

Steward (1955: 88) finds it difficult to derive causal explanations from studying *uniformities* because they are "presumably brought about through diffusion," whereas *regularities* are assumed to be causally interrelated phenomena whose basis is regarded as being linked to adaptation to the environment. Accordingly, he prefers to work with *regularities* occurring in societies sharing a common "culture type." Indeed, I have found in my own study of Central Eskimo that historical factors (among them diffusion) appear to upset attempts at deriving ecological correlations within a culture area. Murdock has selected (in his paper in this volume) the societies in his sample with this factor in mind. The intrusion of historical factors must thus be acknowledged as providing the possibility of overriding ecological considerations and influencing our comparative work in cultural ecology.

But it is the concern with causes which is the essence of Steward's cross-cultural comparison of "culture types." Netting (*see* below) adds the following in this vein:

> The pursuit of causality, whether it is ultimately successful or not, leads to controlled cross-cultural comparisons and historical considerations which are required to make our demonstrations of functional interdependence really convincing.

The search for causal explanations in cultural ecology is really an extension of the functionalist frame of analysis to the problems of environmental – cultural/social relationships. Among the several referents of functionalism used in anthropology (Nadel 1951: 363; Firth 1956), one that applies most directly to cultural ecology is Eggan's (1937: 82) concept of "adaptation" as follows:

> If we look at social systems from the standpoint of an "adaptive mechanism," there are two main aspects: (1) the internal adaptation which is seen in the controlled relations of individuals within the social unity, and (2) the external adaptation of this social structure to the natural environment.

Whereas conventional structural–functional studies are solely concerned with the first of these sorts of adaptations, cultural ecology expands the network of function to embrace another adaptive system, the technological–environmental or exploitative system. To those who espouse a cultural–ecological approach, a high degree of functional consistency is assumed to occur within societies, and the search for relationships between these societies and the habitat or the economy implies that they would also expect that these relationships would be largely explainable in cause-and-effect terms.

But the question arises whether the reference to economic considerations is the sole or even the most important determinant of social phenomena. Helm has noted (in her paper in this volume) that "features of settlement and community pattern can be influenced by ideological factors." If this is the case, it implies that we must be alert to the possibilities of such factors as witchcraft among the Ojibwa as being possible determinants of social features. This implies an expansion of the realm of the functional approach in yet a different direction. Such a broadened frame of functionalism would undoubtedly yield substantial results in outlining interrelationships of a wide scope of cultural and social phenomena. Yet we are cautioned by Nadel (1951: 375) that "there is maladjustment as well as adjustment; the function concept only poses the question of adequacy but does not settle it beforehand" and by Driver (1967: 21) who says "there are thousands of non-functional (historical) relationships in our ethnographies that no one has bothered to assemble."

Whether or not we seek origins of specific features through historical approaches, we must take account of historical factors in cultural ecological studies if for no other reason than (as Netting suggests above) to provide controls for ecological correlations.

Recognition of the limits of conventional structural–functional studies has been one of the important stimuli for the emergence of cultural ecology as a subdiscipline of anthropology. In the same way, recognition of the limits of ecological analysis could encourage a reconsideration of ideological factors and historical factors as determinants of social features in order first to fill the gaps of the functionalist universe, and second to expand the boundaries of explanation beyond it. It occurs to me that the extension of the term 'cultural ecology' to include the latter approaches would reveal an *a priori* conviction that economic factors or features of the natural environment take precedence as determinants of social or cultural features. On the contrary, the relative importance of ecological, ideological, or historical forces must be worked out in detail for each specific feature of each society. If such a field of study, which combines ecological and historical methods, together with attempts to understand the effects of ideology on society, should develop, it seems to me that in its search for origins and in its great breadth it could well qualify as a 'new ethnology.' Such a program would agree in some measure but would also differ in emphasis from the revivals of ethnology suggested by Kroeber's (1953: 366–7) merging of ethnology with culture history and generalizing "the story of culture into its causal factors," or by Eggan's (1954: 759) "combining the sound anthropological concepts of structure and function with the ethnological concepts of process and history."

Whatever the limits of the ecological approach, the papers and discussions of this volume should demonstrate that there are many questions that can be productively treated within that frame of analysis. The quest for functional explanation will continue as the hallmark of ecological analysis. Anthropologists will continue to push back the boundaries of apparently unexplainable phenomena through using ecological approaches and in doing so enrich anthropology by expanding the application of functionalist studies to an exciting and productive level.

NOTES

1. The Eskimo area was also the early site of two other significant attempts to understand culture and society according to relationships with important features of the economy and habitat (Mauss and Beuchat 1905; Steensby 1917).
2. It could be argued that Steward's "cultural ecology" could just as aptly be named 'social ecology' because his core features are more 'social' than 'cultural.' Apart from likely confusion with the ecology of the sociologists, the inclusion of the important cultural ingredient of the technology in the ecological equation probably justifies Steward's designation.
3. For a far more complete review of ecological approaches in all the major subfields of anthropology, *see* Helm 1962.
4. The discussion sessions were transcribed, edited, and sent out to the participants for further changes. A final editing occurred after the transcriptions were returned.
   James G. E. Smith's suggestions for improving this paper are appreciated.

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PART I:

DEVELOPMENT OF THE PACIFIC NORTHWEST PLATEAU CULTURE AREA: HISTORICAL AND ENVIRONMENTAL CONSIDERATIONS

BY

DAVID SANGER

This is a working paper designed to consider the influences of environmental and historical factors in the development of the Pacific Northwest Plateau Culture Area. The Plateau Culture Area, as defined by V. Ray (1939), lies basin-like between the Rocky Mountains on the east and the Cascade/Coast Range mountains on the west. To the north the approximate limit is at the great bend in the Fraser River, while to the south the culture area merges with the Colorado Plateau. The Northwest Plateau is divided into two major physiographic regions: the northern sector, known as the Interior Plateau of British Columbia; and the southern portion, referred to here as the Columbia Plateau after the Columbia River which drains the region.

Environmental Factors

The Interior Plateau of British Columbia is a rolling uplands averaging 3,000–5,000 feet above sea-level with peaks up to 7,000 feet. Deeply incised into the Interior Plateau are two major rivers, the Fraser and its confluent the Thompson. South of the Thompson River is the Canadian Okanagan, a lake system which drains southward through the Columbia River. These rivers provided communication routes from one area of the Interior Plateau to another and in aboriginal times attracted the majority of the area's inhabitants. The valleys of the Fraser and Thompson rivers, together with the Okanagan, comprise the dry grass belt with annual precipitation averaging 15 inches or less. Dominated today by sagebrush (*Artemisia*), cactus (*Opuntia fragilis*), and 'cheat grass' (*Bromus tectorum*), early accounts indicate that bunch grasses (*Agropyron*) were typical valley flora in early historic times (1800–50) prior to overgrazing by introduced cattle and horses (Tisdale 1947). Higher up the valley slopes, the vegetation is less desert-like, with bunch grasses, ponderosa pine, Douglas fir, and eventually open alpine meadows.

Eastern Washington is divided into two physiographic units by the Columbia River. To the north and west are the Okanogan Highlands and the forested foothills of the Cascade Range; to the south is the Columbia Plateau proper, an elevated, loess-covered table land, underlain by numerous basaltic lava flows, dissected by tributaries of the Columbia, and the Snake, and by various dry coulees. Like the British Columbia Interior Plateau, overgrazing of the bunch grass has led to the increasing dominance of sagebrush and *Bromus* in the lower valleys, but in the higher reaches of the northern and western uplands ponderosa pine forests prevail (Daubenmire 1942).

Comparing the gross physiographic features of the two plateaus, it is probably sufficient to note that there are many similarities between the British Columbia Interior Plateau and that portion of Columbia Plateau north and west of the Columbia River. South of that divide the terrain is less rugged, the average annual precipitation is less, and the flora more xerothermic.

From a human subsistence point of view, however, perhaps the most important resource was the tremendous number of salmon (*Onchorhynchus* sp.) annually ascending the major rivers and their tributaries. Salmon not only provided an important source of protein during the summer months but furnished food in dried form throughout the winter. Supplementing the salmon were elk, mountain sheep, the ubiquitous mule deer, and a wide variety of vegetable foods.

Early accounts note the rich salmon resources available to the inhabitants of the Fraser and Columbia rivers, and there is little evidence to suggest that one system was any more productive than the other. Fishing techniques were essentially similar except that dip-net fishing may have been better suited to the more murky Fraser River waters. It seems possible, however, that Indians living in the Interior Plateau and exploiting areas north and west of the Columbia River might have enjoyed superior big game resources. Deer are found in some numbers in tributary canyons draining into the eastern (left) bank of the Columbia, although these hunting territories are limited. The Columbia Plateau proper is not well suited to deer and elk (Dalquest 1948: 108). Vegetable foods are likewise more plentiful in the regions north and west of the Columbia where the greater relief tends to lengthen the growing season of any particular species. The degree of dependence upon vegetable foods probably varied greatly from area to area and year to year, but it seems likely that many archaeologists have underestimated the importance of roots and berries in the total economy of the Plateau people.

During the last major advance of the Wisconsin ice-sheet, the Interior Plateau of British Columbia was heavily glaciated. The fact that striae have been noted at elevations of 6,000 to 7,000 feet suggests an ice-sheet at least a mile high (Duffell and McTaggart 1952: 69). Spreading out of the Okanogan Highlands, ice reached into all but the southern portions of the Columbia Plateau. Probably prior to 11,000 years ago, deglaciation, melt-water ponding, and subsequent drainage patterns changed the morphology of the Columbia Plateau. In the British Columbia Plateau, however, there is no evidence that there was deglaciation until about 10,000 years ago, the earliest date for post-glacial peat bog formation (Fulton, pers. comm.). At this time some of the melt waters from the ablating Cordilleran ice-cap in British Columbia drained south through the Columbia River drainage system, swelling that river to a volume never since equalled. The 10,000-year-old Roadcut site (WS 4) at The Dalles in Oregon, which was apparently located on the then existing shoreline, is now 35 feet above maximum flood stage (Cressman 1960: 67). By approximately 9,300 years ago, however, the bulk of the Cordilleran ice-cap in British Columbia drained west via the Fraser River (Fulton 1965: 88, 96).

Analysis of many bog sections in the Columbia Plateau has indicated to Hansen (1947) that the local post glacial sequence of climatic conditions parallels those recognized in much of the Northern Hemisphere (Deevey and Flint 1957). Comparatively few pollen profiles have been analysed from the Interior Plateau, but those that have exhibit the same general increase of chenopods and composites between 5,000 and 7,000 years ago. According to Hansen (1955), however, the period of maximum warmth and aridity was not so pronounced in the more mountainous Interior Plateau.

One of the main hypotheses of this paper is that the changing post glacial climate played a major role in the developing Columbia River cultures but was of negligible importance to the prehistory of the Interior Plateau of British Columbia. The biotic zones in the Interior Plateau are dependent largely upon altitude. An increase in aridity would conceivably lessen the deer- and elk-grazing potential in the valleys, but at the same time it would increase the range of bunch grass in the higher levels. In short, the upward migration of the plant communities was probably not deleterious; on the contrary, a thinning out of the ponderosa and fir forests along the Fraser River would encourage the presence of elk, generally a more desirable game animal than deer. Finally, any reconstruction of prehistoric climatic conditions in the Interior Plateau should not be based upon the modern flora in the valleys. As documented previously, the presence of sagebrush and cactus is related to early historic overgrazing, which resulted in the introduction of the hardier, more xerothermic species.

In addition to the palynological evidence collected by Hansen (1947), the archaeological record of the Columbia Plateau suggests a significant climatic shift between 8,000 and 4,500 years ago. Daugherty (1962: 145, 147) notes that between these dates the Columbia Plateau assemblages contain food-grinding implements, generally reminiscent of the Desert culture (Jennings and Norbeck 1955).

Although the Plateau groups were not dependent upon salmon to the same degree as the Northwest Coast people were, there is no doubt that salmon was very important in early historic times on both the Columbia and Fraser river systems. Salmon may have been in the Fraser River system since the first well-established presence of man in the region about 9,000 years ago. At the Milliken site in the Fraser Canyon, Borden (1960) has circumstantial but plausible evidence for salmon fisheries at this early date. The earliest skeletal remains, however, come from a site in the Interior Plateau, radiocarbon dated to 7,500 years (Sanger 1967). Salmon remains are noted in other nearby sites, ranging in age from 7,000 years to the historic period. The continuous presence of salmon in the Fraser River system represents an ecological constant which must be considered extremely significant in the overall development of Interior Plateau cultural development.

To the south the situation is more complicated. Here, the earliest salmon remains recovered are from the 10,000-year-old levels at The Dalles. In the more recent levels of the same site, however, all traces of salmon vanish (Cressman 1960: 69). East of The Dalles, but still on Columbia River, Shiner (1961: 190) notes the sudden absence of salmon bones and the increase of river mussel shells in the Cold Springs site. Puzzling this situation, Shiner wonders if, for some inexplicable reason, the annual salmon runs ceased. A search of the published literature, in addition to correspondence with several regional specialists, has failed to reveal any appreciable salmon remains in sites dated between 6,000 and 1,000 years ago.

During the 1965 INQUA conference the ecologist Donald B. Lawrence drew my attention to the Cascade landslide and its probable effect upon the annual movements of salmon. The Cascade landslide, located 60 miles downstream from the present town of The Dalles, Oregon, blocked the Columbia River, creating the Lake of the Gods (Hodge 1938). Radiocarbon dates on two trees

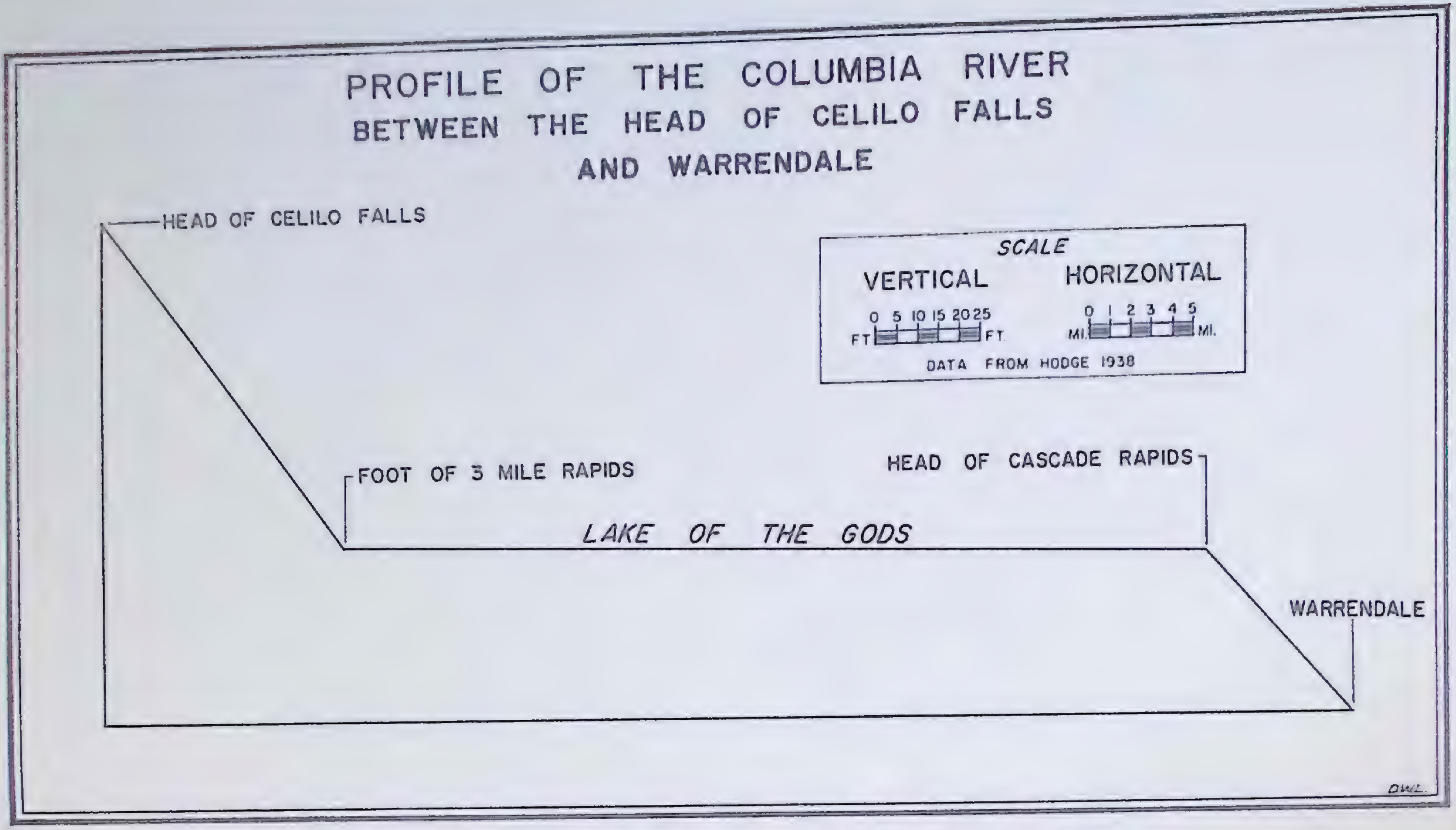

FIGURE 1—Profile of the Columbia River between the head of Celilo Falls and Warrendale.

drowned by the Lake of the Gods averaged A.D. 1265 (Lawrence and Lawrence 1958). The Columbia River evidently had risen over 200 feet behind the landslide dam before it overflowed and began downcutting. By historic times, however, the original gradient of the river had not yet been attained, and the remaining 37-foot drop became known as the Cascade Rapids. As the Columbia River cuts through the Cascade Range, it pours through the Columbia Gorge and over a series of precipitous waterfalls and rapids. At the head of the gorge is Celilo Falls, the famed meeting place of Coastal and Plateau cultures, and the location of The Dalles' archaeological sites. For the next 60 miles the Columbia River falls 123 feet, but 119 of these 123 feet occur in two localities, totalling less than one-third of the distance (Figure 1). Thus the effect of the landslide was the creation of two ladders assisting the salmon over Celilo Falls, which had allowed few fish to ascend in the 5,000 years previous. The drop at Celilo Falls, which the Lawrences (Lawrence and Lawrence 1958: 35) calculate at 60 feet, probably began with the slackening of the high volume of water as the Fraser River began to drain more of the Interior Plateau. This, combined with aridity during the Hypsithermal and normal downcutting, served to create a formidable obstacle to salmon between 6,000 and 1,000 years ago.

In summarizing this brief discussion of the environmental factors, the following points are noted:

1. The Pacific Northwest Plateau is dominated by two main river systems—the Columbia and the Fraser.
2. The progression of moist to arid to modern climatic conditions prevailed, although the contrasts were apparently more pronounced in the Columbia Plateau.
3. The effect upon the resource potential for the prehistoric inhabitants was not equal. It is suggested that the Interior Plateau of British Columbia groups continued to exploit large mammals, vegetable foods, and salmon. The Columbia Plateau inhabitants, on the other hand, enjoyed salmon in early post glacial times, but between approximately 6,000 and 1,000 years ago, few, if any, live salmon got past the barrier at Celilo Falls. The Cascade landslide, dated to about A.D. 1265, made available once again the Columbia River spawning beds.

Historical Factors

According to Cressman (1960), Daugherty (1962), Jennings (1964), and others, the cultural ties of the Columbia Plateau extend to the south and southeast. Two rather distinctive technological traditions and subsistence patterns are known from the earliest period; the bison hunting pattern exemplified by the Lind Coulee site (Daugherty 1956) with probable Plano Tradition projectile point affiliations, and the riverine-oriented system known from The Dalles (Cressman 1960). Both areas date to at least 9,000 years ago, and probably older. Chronologically following these early riverine and big game hunting patterns is the Old Cordilleran concept of Butler (1961). Reduced to its one main diagnostic feature, this complex is recognized by the presence of assemblages with exclusively leaf-shaped projectile points in stone. About 6,000 years ago, or earlier, Mt. Mazama ejected a distinctive ash across much of the Pacific Northwest (Powers and Wilcox 1964). Components stratigraphically above the ash contain a few leaf-shaped points, but large side-notched points with concave to straight bases are more common. Food-grinding implements, basically similar to those found in the Great Basin (Daugherty 1962) are noted here. Between 3,000 to 4,000 years ago the side-notched points were replaced by points with parallel and contracting stems, which in turn were followed by basal and corner-notched points. Throughout this period of close to 7,000 years the Columbia Plateau is thought to have participated in the Western Intermontane Tradition, which, according to Daugherty (1962), extended from British Columbia to Mexico. It is only after A.D. 1 that the Columbia Plateau developed into the Northwest Riverine Tradition (Daugherty 1962; Jennings 1964) with an economy based upon intensive utilization of the river resources.

Swanson (1962) proposes a sudden cultural efflorescence on the Columbia River in what he calls the "emergence of Plateau culture." He speaks of a sudden reorientation of subsistence, settlement, and community patterns in the form of intensive riverine exploitation and the appearance of housepit dwellings arranged in villages. This emergence is dated between A.D. 1200 and A.D. 1300.

The prehistory of the Interior Plateau of British Columbia, as it is presented here, is based upon one lengthy and nearly continuous cultural sequence and a series of scattered sites. The sequence, which is from the Lytton–Lillooet region on the Fraser River in the heart of Upper Thompson ethnographic territory, indicates occupation for at least 7,500 years—the oldest radiocarbon date thus far received (Sanger 1967). From the very beginnings of the sequence there are significant dissimilarities with assemblages of comparable antiquity on the Columbia River. The Lytton–Lillooet projectile points are quite distinct from those to the south and indicate closer relationship to point styles known in the Canadian Prairies and north-central British Columbia. Assemblages from British Columbia also contain microblades and microblade cores from the 7,500-year-old component until about 2,000 years ago. At times microblades constitute 50 per cent of all artifacts in assemblages. Analysis of the microblades and, more important, the microblade cores indicates that the closest similarities exist with certain microblade cores designated "tongue-shaped" by Irving (1962), distributed north through the Yukon, Alaska, and into Asia. To date only two microblade-bearing components have been reported from the Columbia Plateau. Another major distinction involves habitations. About 3,500 years ago the first known semisubterranean housepits appear in the Interior Plateau. With one or two exceptions (Daugherty *et al.* 1967; Grabert 1966), housepits in the Columbia Plateau are a feature of the Christian era, with the majority dating to the past 1,000 years.

Thus the prehistoric Columbia Plateau cultures may have shared many traits with the various Desert Cultures to the south until about A.D. 1000, when certain traits, long associated with the Interior Plateau of British Columbia, make an appearance. These include the housepits, the well-developed woodworking complex, and an intensified riverine orientation.

Two other lines of evidence reinforce the archaeological interpretation. In defining the Plateau Culture Area, Ray (1939) notes the presence of certain divisions. Based largely upon non-material elements of culture, Ray documents a cultural break at about the present International Boundary. The linguistic division between Interior Salish and Sahaptin speakers, however, is 180 miles to the south. Suttles and Elmendorf (1962), and more recently Elmendorf (1965), have discussed the Plateau Culture Area with reference to linguistic, cultural, and ecological boundaries. The culture of the Interior Salish area, which is considered to be adapted to the forested valleys of British Columbia, is thought to have moved south with the onset of the moister Medithermal climatic phase, *circa* 4,000 years ago. Present archaeological evidence suggests that this date may be too early, however, for the southward diffusion of cultural traits associated with the Interior Plateau.

In summary, the evidence presented here points to a long-term separation between the two portions of the Plateau, with the Columbia Plateau affiliating with the Desert Cultures and the British Columbia Interior Plateau sharing traits with the Canadian Prairies and the Subarctic. The observed similarities in ethnographic times are apparently the result of a late (*circa* A.D. 1000) influx of Interior Plateau cultural traits, including Interior Salish languages.

Three dates, each derived independently, coincide in a suggestive manner. First, there is the A.D. 1000 date for the recognition of shared traits including housepit villages and woodworking implements. Second, there is Swanson's intensified riverine orientation and "emergence" dated to A.D. 1200–1300. And finally, there is the A.D. 1265 date for the Cascade landslide and the reintroduction of salmon into the Columbia Plateau.

Discussion

The series of environmental and historical events leading up to the historic Plateau Culture Area may now be summarized and integrated. In the Columbia Plateau the earliest inhabitants of which we have any record were evidently making extensive use of the salmon resources. But around 8,000 to 6,000 years ago the salmon exploitation pattern weakened, and it was at this time that contacts with the Desert Culture of the Great Basin intensified. The loss of salmon may have been due to the lessening of the water volume during the Thermal Maximum, which combined with downcutting to create a barrier at Celilo Falls. With the general dessication and loss of salmon, the Columbia River groups may have been obliged to reorient their subsistence patterns, adapting them to techniques already well established in the desert regions to the south. Then a basic change in subsistence and settlement patterns, equated with Swanson's "emergence of Plateau culture," occurred at *roughly* the same time that salmon once again began ascending the Columbia River past Celilo Falls. Coincident with this was an influx of Interior Salish speakers from British Columbia. The great importance in historic times of The Dalles as a meeting place of Coastal and Interior cultures may well have its roots in the old upriver limit of the salmon. Shortly before and during the early years of the historic period, influences from the Chinook and from the Plains cultures strongly affected the Columbia Plateau cultures.

The Interior Plateau of British Columbia was probably populated initially by groups coming out of the deglaciated Columbia Plateau. Artifacts of general northern affiliation, such as microblades, appeared by 7,500 years ago. Contacts with the western Canadian Prairies were evidently maintained from this period until historic times. In the last 2,000 years British Columbian groups, now speaking Interior Salish, pushed southward, possibly through the Okanagan Valley. The Interior Salish brought with them a culture adapted to the riverine resources and the forested hills of the Fraser drainage system. Near the end of the prehistoric period the Interior Salish were influenced by ideas originating from the Northwest Coast and from the Plains.

At our current state of knowledge concerning the prehistory of the Northwest Plateau, the foregoing discussion appears to include some of the most significant historical and environmental factors. The implications for the early historic Plateau societies have yet to be worked out in detail, but several observations seem pertinent.

The speed with which the Columbia River groups reoriented their subsistence, settlement, and community patterns is of interest. If Swanson's dates of A.D. 1200 to A.D. 1300 for the "emergence" are correct, it would seem that reorientation following the Cascade landslide and the renewed presence of salmon in A.D. 1265 was almost immediate. It does not appear as if the Columbia River peoples were making a slow trial and error type of adjustment to the new riches of the river. Patterns previously worked out in nearly six millennia of experimentation on the Fraser River may have been borrowed with only slight adjustments.

Ray (1939) notes that it is in religion and what he calls social attitudes that the Columbian Plateau groups exhibit the greatest divergence from the British Columbia societies. Recently I have compared artifact lists from late prehistoric sites in the two sectors of the Plateau and have come to the conclusion that the most striking differences do in fact concern artifacts which are not primarily associated with subsistence techniques (Sanger 1969). Here may be a case of long-standing religious and social attitudes being little influenced by a rapidly changing environment. Subsistence and settlement patterns, on the other hand, may have been substantially altered.

Finally, housepit villages appeared in the Interior Plateau of British Columbia about 3,500 years ago, whereas the settlement pattern might be a feature of the past 1,000 to 1,500 years only in the Columbia Plateau. It seems possible that these long-term differences in settlement pattern have had important implications for developing community patterns, kinship, and political systems in the two plateaus and may offer a partial explanation for the dichotomy documented by Ray.

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NATURAL ENVIRONMENT - SOCIAL ORGANIZATION - WITCHCRAFT:

CREE VERSUS OJIBWA — A TEST CASE[1]

BY

Edward S. Rogers

Introduction

The relationship between man's culture and his natural environment has for many years been an area of interest. Ratzel first examined this problem critically within the context of anthropogeography (1896–98). From this beginning, Huntington (1959) and Semple (1911) carried it to the extreme with the theory of 'environmental determinism.' As late as 1960 Wagner wrote:

> The purging of such 'environmentalistic' thought from academic geography has been a long and painful process from which we are still only convalescing. (1960: 5)

However, the French maintained a moderate view, as best expressed in the work of the cultural geographers de la Blanche (1950) and Brunhes (1952).

The extreme views expressed by Huntington and especially Semple led ethnologists to neglect the question of the relationship between culture and environment. Writers like Boas even opposed any such approach:

> . . . it would seem that environment has an important effect upon the customs and beliefs of man, but only in so far as it helps determine the special forms of customs and beliefs. These are, however, based primarily on cultural conditions, which in themselves are due to historical causes. (1924: 162–3).

Nevertheless, interest never completely died, and Wissler (1926), Kroeber (1947), and Steward (1955) did much to revive it.

Today it has become fashionable to speak in terms of 'cultural ecology,' a combination of terms defined and perhaps first used by Steward (1955: 30ff.). Now it appears that explanations in terms of environmental conditions and technological levels are again approaching an extreme position.

Turnbull, after intensive work with the Mbuti pygmies had this to say:

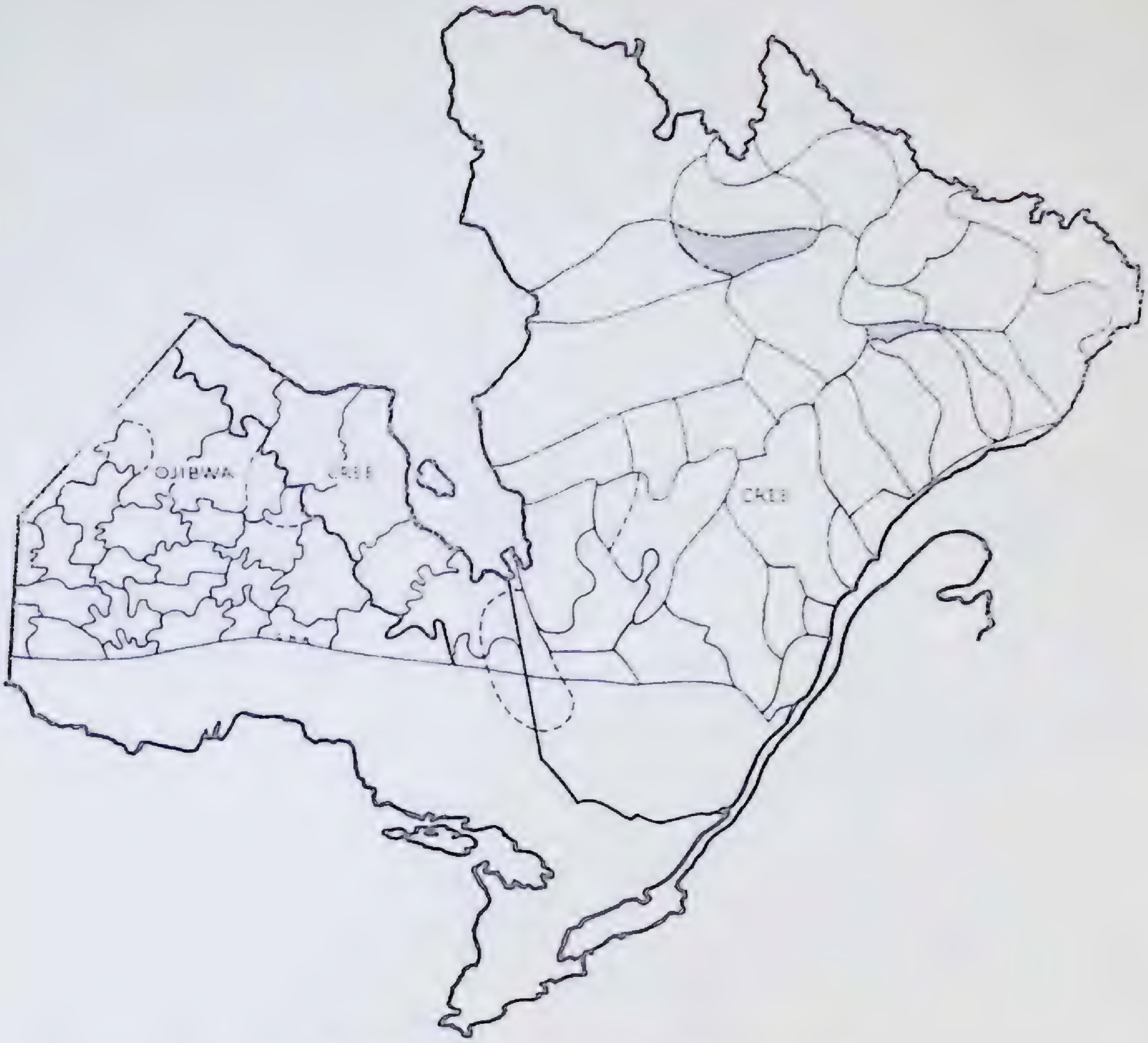
> While agreeing with Steward that the environment may to some extent be determinant as well as persuasive of certain elements of social organization, I find it impossible to see the validity of his wider deterministic claims for cultural ecology. (1965: 277)

But he does add:

> . . . the environment is determinant of their value system so long as that dependence [i.e., on the environment] is felt. (1965: 278) (Brackets mine)

In the present paper, I am surveying the social organization of the Cree and Ojibwa peoples who inhabited (and still inhabit) the Eastern Subarctic of Canada (Map 1) in relation to their natural environment and the practice of witchcraft. I hope to show that no strictly environmental explanation can account for their social organization throughout the entire area.

Much of the data for this paper is based on the results of field-work among the Mistassini Cree (1953–54), the Round Lake Ojibwa (1958–59), the Parry Island Ojibwa, Potawatomi, and Ottawa (during the summers of 1963, '64, '65,



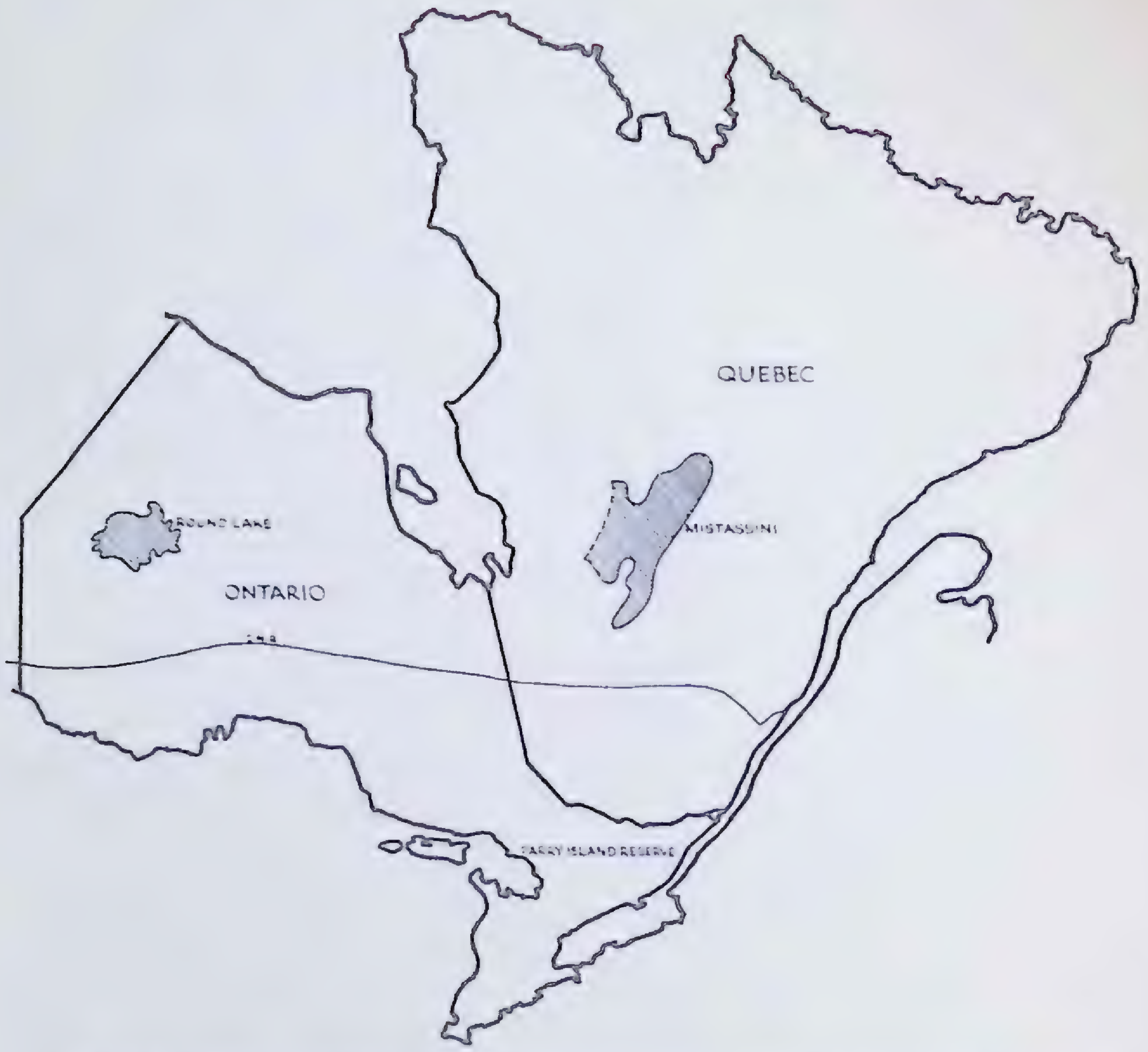
MAP 1—Distribution of Cree and Ojibwa

and '66) (Map 2); short visits to other reserves within Ontario; and talks with Indians from all parts of the Province. Other data were secured from published sources.

The paper is organized as follows: first, the natural environment is considered, followed by the economic and social organization of the Cree and Ojibwa; then witchcraft is considered as it occurred among these people; and finally, conclusions are drawn.

The area under consideration extends from the Atlantic, along the Labrador Coast, and west to approximately the Manitoba border. It is bounded on the north by Hudson and James bays and Hudson Strait and on the south by the St. Lawrence and Ottawa rivers and the north shore of lakes Huron and Superior.

The Cree inhabited all the Labrador Peninsula, exclusive of those coastal areas occupied by the Eskimo. The Cree extended west around the southern tip of James Bay, through the Hudson Bay Lowland to Northern Manitoba and beyond.



Map 2—Location of Mistassini and Round Lake bands and Parry Island Reserve

The Ojibwa inhabited the Laurentian Upland of Ontario and extended south into Wisconsin and Minnesota. It is true the Cree extended west beyond the area boundaries given, and after contact both Cree and Ojibwa advanced into the Plains area. These latter groups are not of direct concern in this paper.

The Natural Environment

Basically, the natural environment is and was similar throughout the entire area under consideration. The region encompasses a part of the Circumpolar Boreal Forest and is part of the Precambrian Shield. The landscape is of low relief in most areas, except in the Central Labrador Peninsula and along the Labrador Coast. In these latter sections, elevations rise to several thousand feet above sea-level. Much of the area is covered with glacial debris as a result of the last glacial advance and retreat. This period of glaciation greatly affected the topography, in that it disrupted the drainage systems and created lakes, ponds, and extensive areas of muskeg. These latter are especially characteristic of the Hudson Bay Lowland section.

The climate is continental with long, cold winters and relatively short, warm summers. Precipitation is moderate with fifteen to forty inches annually.

Snowfall is heaviest within the Labrador Peninsula, where it may amount to as much as one hundred inches or more. Winds are not of great importance since their force is broken by the forest cover. The land is blanketed with snow, and the lakes, ponds, and rivers are frozen from approximately October until May.

Coniferous forests cover much of the area except in coastal tundra regions and within the Hudson Bay Lowland where extensive areas of muskeg support only a limited number of trees. The trees that compose the forest are black and white spruce, balsam fir, Banksian pine, white birch, and common and balsam poplar. Alders, willows, and other shrubs are numerous, especially along stream banks and shores of ponds. A variety of berry bushes exist, the most common being the blueberry. The forest floor is often covered with a thick matting of mosses and, in more open areas, with lichens.

The fauna does not contain a large number of different species. Among mammals, there exist caribou, moose and coyote in recent years, bear, beaver, lynx, otter, mink, marten, muskrat, hare, ermine, fox, wolf, wolverine, and some smaller species. As for birds, waterfowl are the most important, and a variety of species of ducks, geese, and loons can be found. These migrate through the area in the spring and fall. The main migration route is by way of James Bay, along the coast of the Hudson Bay Lowland. Inland, ptarmigan and spruce and ruffed grouse were important species to the Cree and Ojibwa. These birds remain all year, not being migratory to any great extent, as are ducks and geese. In addition are found owls, hawks, and many song birds which summer in the area.

There are a number of different species of fish which inhabit the area. Among those of importance were pike, lake trout, whitefish, sturgeon, walleyed pike, speckled trout, ling, and, in the western part of the area, perch, goldeye, and sauger. In addition, there were a number of smaller species, but these were of no direct value to the Indian.

Insects are numerous, and the most annoying are the vast numbers of mosquitoes and black flies during the summer months.

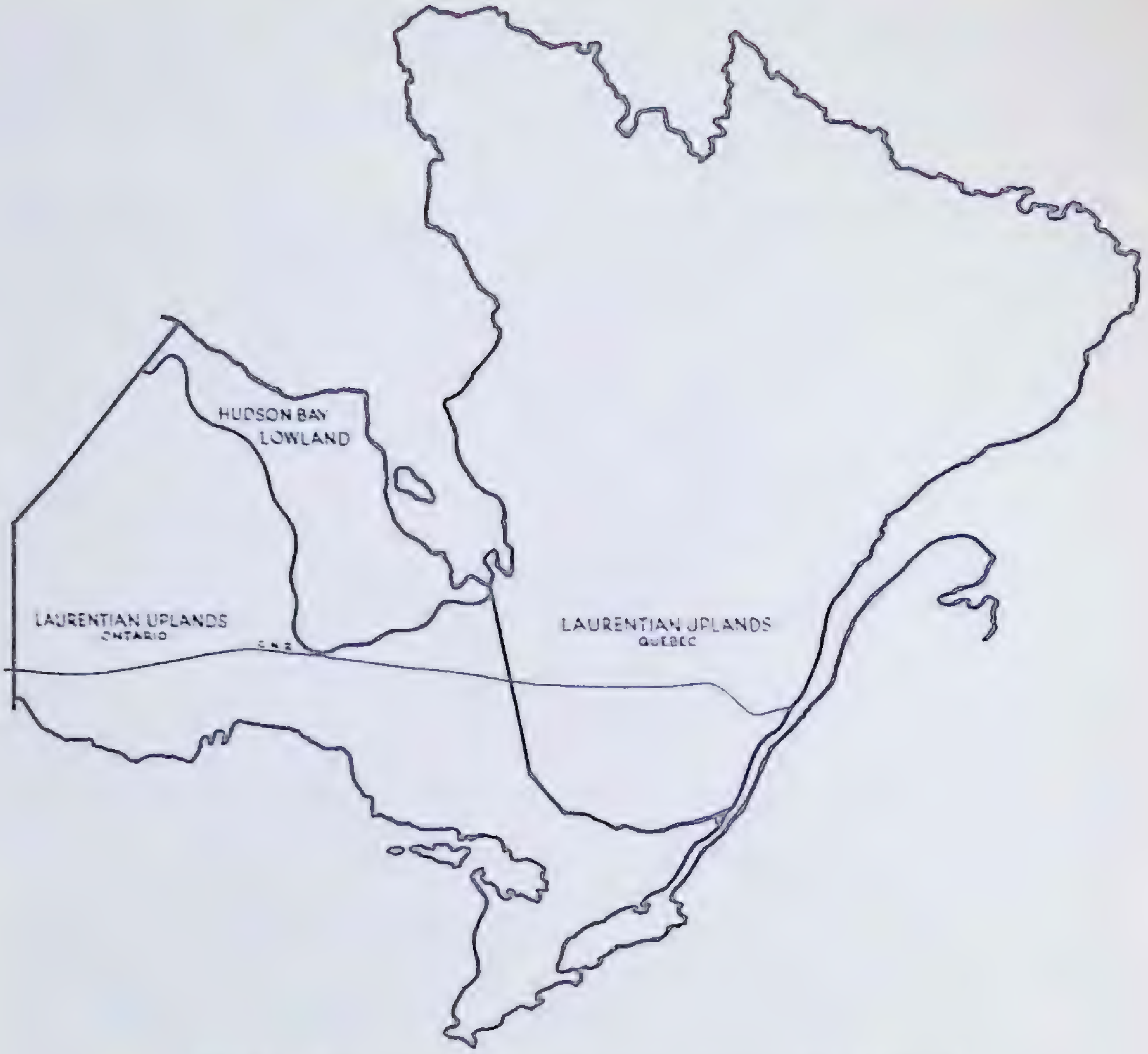
Although there was considerable similarity in the natural environment throughout the entire area, two major sub-regions can be delineated on the basis of their biotic potentials. These must be considered for a proper understanding of the social organization of the Cree and Ojibwa.

The first sub-region comprises the Laurentian Upland of Quebec and the Hudson Bay Lowland (Map 3). The most notable feature of this sub-region is the tremendous paucity of resources. Accordingly, the area had in the past, and still has, a very small human population. It is one of the most sparsely populated areas to be found anywhere in the world.

The second sub-region is the Laurentian Uplands of Ontario. There, greater densities of resources exist and, in addition, several species that do not inhabit the other sub-region. Accordingly, in the past the human population was denser than in the Quebec Laurentian Upland – Hudson Bay Lowland sub-region, and it is still true to this day.

Economic Organization of the Cree and Ojibwa

The economic system of the Cree and Ojibwa was relatively uncomplicated. They lived by hunting, fishing, fowling, and trapping. Gathering was minimal.



MAP 3—The Laurentian Uplands versus Hudson Bay Lowland

In the extreme southern part of the area they practised some horticulture, gathered wild rice, and collected maple sap. The importance of the fur trade varied between 1650 and 1850, depending upon the area under consideration (Rogers 1964).

With the exception of wolves, foxes, and extremely small animals, most species of mammals, birds, and fish were used as food by the Indians. Moose (recently), caribou, bear, beaver, and fish were the most important source of food within the Laurentian Upland of Quebec. On the Hudson Bay Lowland, waterfowl and fish were the staple of the diet. On the Laurentian Uplands of Ontario, fish increased in importance as an item of food. Hare, in recent years at least, have been utilized extensively. Of course other species were also taken.

Within the area, faunal resources have changed over a period of time and concomitantly so has the degree of utilization of various species by the Cree and Ojibwa (Rogers 1966). Initially, except within the Hudson Bay Lowland, the people appear to have lived on caribou and fish, but with the coming of the fur trader changes took place. Small game, along with fish, became important food resources to the Indians in the southern part of the area. At a later date, towards the beginning of the twentieth century, moose began to move into the area and by now are resident throughout most of the territory (Peterson 1955: 14–18).

In order to exploit these resources, the Cree and Ojibwa had a definite pattern of movement during the course of the year (Rogers and Rogers 1959). During the summer, the total population of a band gathered within their territory on the shores of a lake that was productive of fish. With the approach of fall, the band broke up into hunting groups, which moved to individual hunting areas within the band territory. There, as a rule, they spent the entire winter. If the hunting area was large, then the group might move camp one or more times during the course of the winter. This was especially true in the eastern part of the area. With the coming of spring, the hunting groups began to reassemble at the summer encampment.

The methods used by the Cree and Ojibwa peoples of the Eastern Subarctic to exploit the natural resources of the area were relatively simple (Rogers 1967). For hunting and trapping game animals, they used bows and arrows, deadfalls, snares, and, to a limited extent, the chute and pound for caribou. The latter device was used only by those bands in the Central Labrador Peninsula (Rogers 1967; Skinner 1911: 25–7). As the fur trade expanded throughout the area, these methods of securing game were soon replaced by guns and steel traps.

Fish were taken with spears and hooks and in weirs by some bands. Later it appears that gill nets were introduced and soon became the major means of securing fish.

Birds, especially waterfowl which were of importance on the Hudson Bay Lowland, were taken with bow and arrow from behind blinds, placed either on shore, at the bow of a canoe, or in traps and snares. Decoys were used in various parts of the area to attract waterfowl within range. Again, in time, guns, especially shotguns, replaced bows.

The acquisition, distribution, and consumption of the resources of the area were controlled by a limited number of individuals. In the securing of resources, it was the hunting group and the nuclear family which were of most importance. The band as a unit was never involved except, occasionally, among those bands in the extreme northern part of the Labrador Peninsula; and this was done only for caribou hunting.

The men, generally all those present in the hunting group, hunted caribou together, and occasionally moose. At other times, they hunted and trapped in pairs.

The nuclear family was important in securing fish and occasionally in limited hunting and trapping endeavours.

Sometimes, however, an individual alone, either a male or female, might hunt for birds or set traps within the vicinity of the winter camp.

In general, distribution took place within the nuclear family. On the other hand, big game was shared among all the families of the hunting group regardless of who actually secured the animals. In addition, gifts of country food, other than big game, were on occasion distributed to other members. Another means of food distribution was by feasts, held periodically, to which all the members of the hunting group were invited. This, also, sometimes took place at the band level during the summer months.

The nuclear family primarily consumed the game. On the other hand, game was occasionally shared among the entire population of the hunting group and sometimes among the band at feasts.

Social Organization Among the Cree and Ojibwa

The nuclear family and the extended family are not the direct concern of this paper. They should, however, be mentioned. The nuclear family consisted of a husband and wife with unmarried children and any dependants, usually close relatives. This equates with what Dunning has termed the "Commensal Unit" (1959: 63–4) and what I have termed the "Household" with minor exceptions (Rogers 1962: B65–B69). The nuclear family, as here constituted, was found throughout the area. It was a stable unit except after disruptions caused by deaths and marriages. Furthermore, the dependent individuals might on occasion move in with another nuclear family.

The extended family was found throughout the area but appears to have been somewhat more highly structured among the Ojibwa. It was based on kinship but might coincide with the hunting group, which was based on territoriality. Among the Ojibwa, the extended family had a depth of three generations. It was led by a senior male who was in charge of his younger brothers, married or not, unmarried sisters, and all married sons of his own, and his brothers and their families, along with any unmarried sons and daughters. Such a unit exchanged females with similar units through marriage. If the woman's spouse died, she returned to her natal group and was again reapportioned through marriage. This was the ideal, although it was seldom possible to achieve because of differential mortality of the sexes, which upset the ideal situation.

Besides the nuclear and extended families there were two social units of concern to this paper. These are the hunting group and the band.

The hunting group, found throughout the area, was based on territoriality, although the member families were generally closely related. Such a group spent the winter months together and occupied a winter camp. This unit is equivalent to Dunning's "Co-residential Group" (1959: 55). Ideally, it was composed of a man and his three married sons, in which case it could be equivalent to the extended family, but this was rarely so. Often other relatives were involved in the composition of the group and at times with individuals who could trace no kinship connection with other members.

When hunting and trapping, the men either worked as a collective unit or in pairs. There was a reluctance to hunt individually any more than necessary, especially when it meant being away from the base camp for more than a few days.

Hunting group size varied slightly. There were approximately twenty individuals within each hunting group, but this was not always possible because of fluctuating population densities within any segment of any given band at any particular time and as a result of differential mortalities that disrupted sex ratios.

Accordingly, the hunting group varied in size from approximately seven to twenty-five individuals.

Table 1

Band[1]	Hunting group size (number of persons)	
	Range	Average
Mistassini	6–25	15.0[2]
Lake St. John	9–39	10.2[3]
Waswanipi	4–24	7.0[4]
Grand Lake	2–23	6.0[5]
Tête de Boule	1–13	5.8[6]
Abitibi	4–28	7.0[7]
Eastern Cree	10–20[8]	
Attawapiskat	10–25	17.0[9]
Round Lake	1–12	7.0[10]
Pekangekum		20–22[11]
Berens River	4–49	14.9[12]

1. *See* Map 4 for location of these bands within the Eastern Subarctic.
2. Rogers 1963: Table 7, 57.
3. Speck 1927: 396, 399–402.
4. Davidson 1928: 46, 47, 57–9.
5. Davidson 1926: 91, 93, 95.
6. Davidson 1928*a*: 37–45.
7. Jenkins 1939: 30–1.
8. Skinner 1911: 57.
9. Honigmann 1956: 58.
10. Rogers 1962: B76.
11. Dunning n.d.: 2.
12. Hallowell 1949: 40.

Hunting groups tended to be scattered equidistantly throughout the band territory during the winter months. In the Labrador Peninsula, the distance between encampments was as much as fifty miles (Rousseau 1949: 36). In the West, it was as little as five to ten miles, or even less, between camps was the rule.

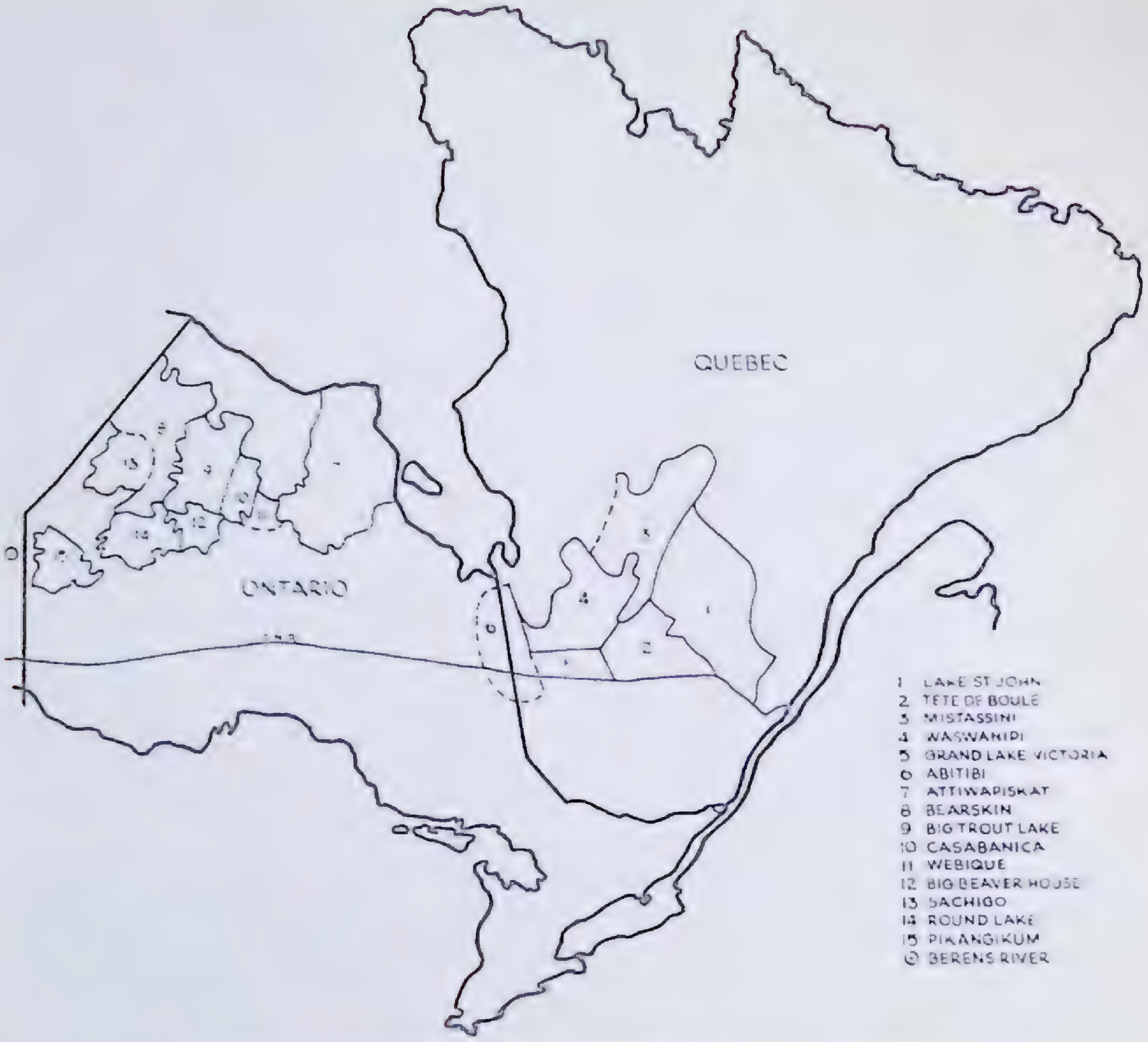
The other social unit of importance was the band. In a previous paper (Rogers 1969), I discussed in some detail the concept of the band as it existed and exists today among the Cree and Ojibwa of Eastern Subarctic Canada. Accordingly, there is no need here to repeat in any detail data presented there.

Each band was composed of a number of hunting groups. The band, as a rule, only came together in face-to-face relationships during the summer months when its members gathered on the shores of a particular lake favourable for fishing. With the approach of fall the band broke up into its constituent hunting groups which scattered throughout the adjacent territory for the winter months, as a rule, not to reassemble again until late spring or early summer.

The band was under the guidance and 'jurisdiction' of a so-called 'leader.' He had, however, relatively little power and was, in effect, a charismatic leader. He had religious power, perhaps more than any other male within the group, which helped him maintain his position. He was, in a sense, more of a councillor than an autocratic headman (Rogers 1965).

Band size throughout the Eastern Subarctic among the Cree and Ojibwa was roughly constant. It ranged from a minimum of approximately 50 to 75 to a maximum of 125 to 150 individuals. It appears that it was somewhat larger along the southern margins of the area.

A complicating factor in determining band size was the establishment of trading posts by various fur companies. These posts became centres about



MAP 4—Location of certain bands of the Eastern Subarctic

which the Indians gathered, not necessarily according to previous band membership. Trading posts were fewer in number than were the aboriginal fishing sites where bands formerly gathered, and, therefore, the posts tended to attract larger aggregates.

Although, in general, band and hunting group size was relatively constant throughout the Eastern Subarctic, the density of population was not. Within the Labrador Peninsula, population density was well over 50 square miles per person and probably in the neighbourhood of 100 or more square miles per person. Elton estimates for this area about 125 square miles for each person and perhaps over 500 square miles for each hunter (1965: 250). Farther west toward the Manitoba border, the density of population was much greater, and in the extreme western part of the area it was six square miles or less per person (Figure 1).

As pointed out, the social organization of the Cree and Ojibwa of Eastern Subarctic Canada and the size of bands and hunting groups were quite similar throughout the area. Yet, farther west the population density increased rapidly.

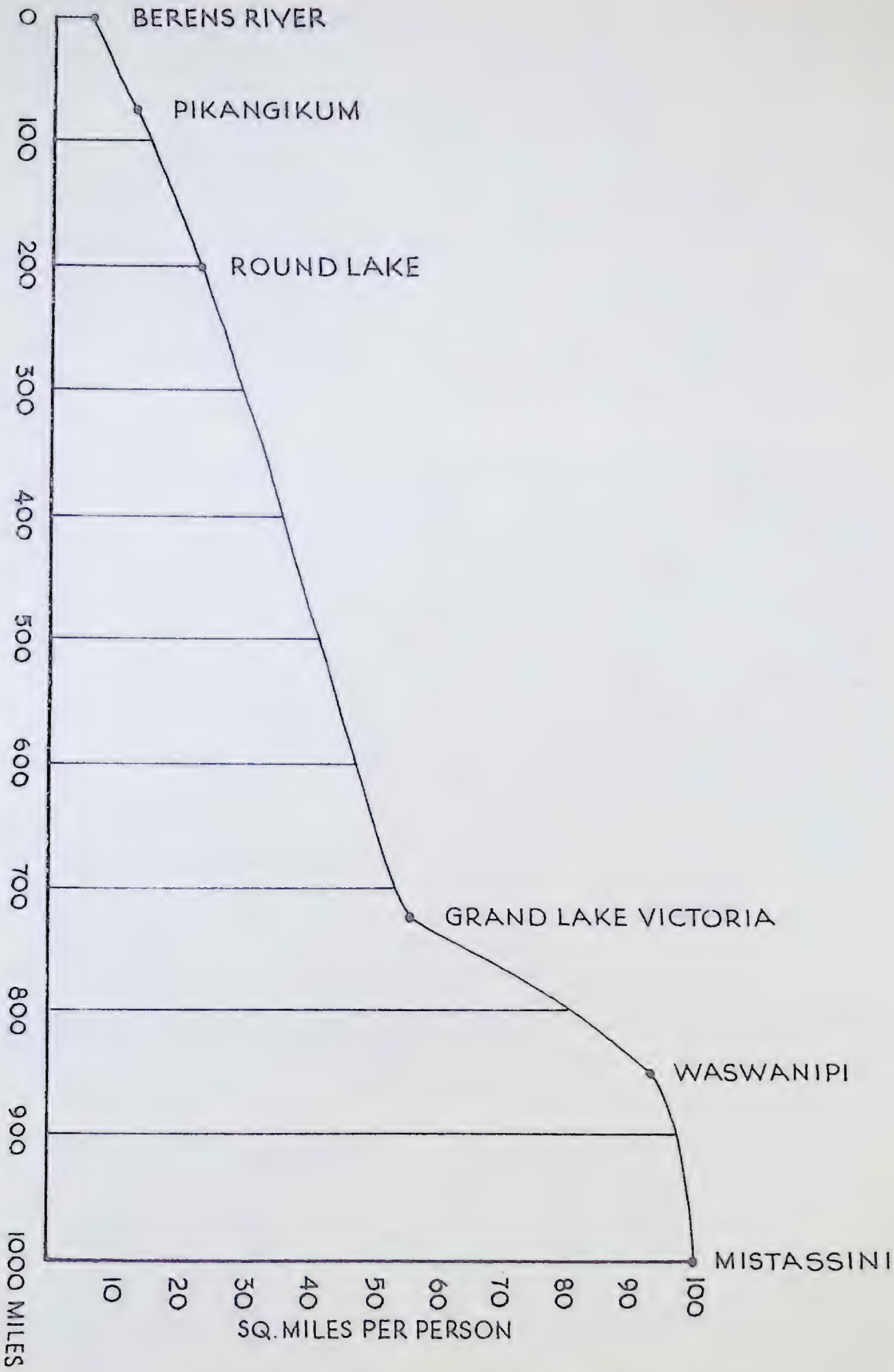


FIGURE 1—Population densities in the Eastern Subarctic

Table 2

Band[1]	Population density in sq. mi./person
Mistassini	66.0[2]
Waswanipi	93.0[3]
Grand Lake Victoria	55.6[4]
Round Lake	22.0[5]
Pekangekum	12.5[6]
Berens River	6.2[7]

1. *See* Map 4 for band locations.
2. Rogers 1963: 22.
3. Davidson 1928: 46, 47, 57–9.
4. Davidson 1926: 91, 93, 95.
5. Rogers 1962: B2, B7.
6. Dunning n.d.: 2.
7. Hallowell 1949: 40.

The question then is why, as population density increased, did not the size of the hunting group and (or) the band do likewise? At Mistassini, for example, because of the limited transportation facilities available, the maximum extent of territory that could be exploited effectively by one hunting group, regardless of its size, was 1,500 square miles. But such an extent of territory because of low game densities would support no more than 15 to 20 people. This, of course, is at one extreme. At Berens River, on the other hand, group size averaged 14.9, the same as the Mistassini, but the territories exploited were much smaller. Accordingly, if with the technology possessed by the Cree and the Ojibwa which enabled 1,500 square miles to be effectively exploited, why were not larger groups found exploiting larger territories at Berens River? There are no known strictly environmental factors or technological limitations to inhibit the growth of hunting group size.

One might suggest that in the western part of the area, fish and small game were important in the subsistence quest and that these, being widely scattered, were most effectively exploited by small groups. Or it might have been that it was more effective to exploit the environment, regardless of the game taken, by means of small groups, thereby reducing the amount of energy used in traversing the territory. It also may have been that the Indians had no adequate distribution system by which the game taken by one segment of a large group could be shared with the rest of the members. The lack of effective overall leadership could be involved here. These may be factors which explain small group size in the western part of the area, but they do not seem to be, in themselves, sufficient reasons.

It is interesting in this connection to examine the summer settlement patterns as they exist within the Eastern Subarctic at present. Among the Cree of the Labrador Peninsula and Hudson Bay Lowland and among several bands (Sachigo, Bearskin, Big Trout Lake, Casabonica, Big Beaver House, and Webique) (Map 4), on the northeastern edge of the Laurentian Uplands of Ontario, the homes in the summer settlements are placed close together; yet there is no geographical reason for this. Among the other bands, all Ojibwa summer settlements are strung out along a lake shore with as much as a mile or more between homes.

Accordingly, it is suggested that the role of witchcraft was of extreme importance and still is to this day in determining group size and in the arrangement of settlements among the Ojibwa of the western part of the area.

Witchcraft Among the Cree and Ojibwa

Witchcraft was found throughout the Eastern Subarctic among both the Cree and the Ojibwa. Among the Cree, however, witchcraft was of limited concern to the people. Rarely have any instances been recorded. Ethnologists such as Trudeau, Honigmann, Speck, McGee, Cooper, and Flannery, who have worked in the area, have not found any great interest in witchcraft on the part of the people. In early accounts, there is only the very occasional mention of feuding in the context of shamanism (Thwaites 1896–1901, 6: 195–9; Le Clerq 1881: 192–4). Speck (1935: 49) and Lips (1947: 482) do mention rivalry between shamans in the present century. And at Mistassini one case was recorded (1953–54) of a shaman who bewitched a girl for refusing to marry him.

Cree religious rituals, to a major degree, revolved about the food-quest – game-animal feasts, proper treatment of game remains, dreams, various forms of divinatory practices, and drumming for game. The food quest was uppermost in the minds and thoughts of the Mistassini. Starvation always threatened. Whenever telling stories, Mistassini informants usually talked in terms of food and the harshness of the environment. The latter made it difficult to seek and find sufficient resources to support life. They were not particularly concerned with hostile interpersonal relationships. They were not fearful of each other, anxiety-ridden, or distrustful of their neighbours. In fact, mutual aid was highly developed (Lips 1937).

Among the Ojibwa, on the other hand, this was not so. Interpersonal conflict was extremely common (Rogers MS. The northern Ojibwa, "cooperative or competitive"). The food quest, while of importance, was not of overriding concern. Although the Ojibwa were obsessed with interpersonal relationships, as attested by travellers and later by ethnologists (*see* Hallowell 1942), they were not given to physical violence except among themselves when drinking. As Hallowell has said:

> . . . absence of overt aggression when in face-to-face situations is an outstanding feature of interpersonal relations in Saulteaux society. (1955: 278)

Instead, feelings were suppressed. An Ojibwa was fearful of his neighbour, suspicious, anxiety-ridden, and hostile.

Landes remarked,

> . . . but there is great anxiety and hostility. It is as though the Ojibwa can never free himself from the awareness proper to the winter, and at all times uses the proximity of other Indians as a threat. (1937: 57)

Landes further said:

> Ojibwa life may be thought of as resting on three orders of hostility. All Ojibwa-speaking persons feel a major hostility toward those of alien speech, epitomized by the Dakota Sioux. Next in order is the hostility that exists between different local groups of Ojibwa, and the third is the hostility felt by any household towards another, whether or not of the same village.
>
> Politically and economically the Ojibwa are an atomistic society. (1961: 102)

Mead commented on these remarks as follows:

> The most conspicuous instance of correspondence between the natural environment and the cultural emphasis is to be found among the Ojibwa. Their highly individualistic way of life is completely congruent with the sparse distribution of game animals which makes it necessary for men to scatter widely for several months of the year.

Nevertheless, she was puzzled, for she added:

> But it still remains a problem why the habits of the winter months should so completely dominate their whole outlook, rather than the habits of the summer months when they collect in villages. (1961: 464)

The answer to Mead's dilemma appears to lie in the practice of witchcraft. Jenness wrote of the Parry Island 'Ojibwa':

> It is pathetic to observe how universal is this fear of witchcraft among the present inhabitants of Parry Island. Every man suspects his neighbour of practising the nefarious art to avenge some fancied grievance, and the older and more conservative the Indian, the more he is held in suspicion. Probably there is not a single adult on the island who has not been accused of sorcery at some time or other, and who has not himself suffered some misfortune which he attributes to the same cause. (1935: 87)

Barnouw stated:

> It seems evident that the atomistic nature of this social structure (i.e., Ojibwa) was reinforced by certain attitudes of fear and suspicion engendered through the process of child training and continuing through adult life. Children were disciplined by scaring techniques and taught to be afraid of owls, bears, snakes, ghosts, white men, Indian enemies, menstruating women, medicine men, and people in mourning. Particularly significant was the widespread fear of sorcery, which multiplied mistrust and inhibited social interaction. (1950: 72) (Brackets mine)

Hallowell felt similarly regarding witchcraft:

> This is the psychological explanation, it seems to me, of the "atomism" or individualism, of Ojibwa society . . .

And he goes on:

> It is impossible for people to get together when their outlook is coloured by the possibility of malevolence, particularly when there are no social institutions that demand a high degree of cooperation. (1955: 147)

The Round Lake Ojibwa of Northern Ontario exhibited in the past a similar pattern of hostilities, anxieties, suspicions, and fears between individuals. Statements such as the following were typical of their sentiments regarding their former life.

> In the old days people were drumming, dancing and medicine.

(This statement refers to the practice of witchcraft.)

Referring to troubles over women, one informant stated:

> Sometimes the two men fight, sometimes one is killed. Maybe use shaking tent to do this.
>
> Everything themselves the bad things, killing people — using the shaking tent.
>
> In the old days men were mad at each other and took things from each other. Sometimes take the wife or they might use the shaking tent in one camp and people die in the other. Then this camp mad at the other camp.
>
> Long ago a man goes on his trap-line and finds beaver. Then someone come and kills that beaver and then that first man mad. Sometimes getting angry that man and doing the witchcraft, drumming and shaking tent for this.
>
> Sometimes in the old days a man asks another for his daughter but the latter man refuses. Then the former gets mad and tries to kill the girl's father with a shaking tent.
>
> Formerly bad medicine men killed people.

Even today these same attitudes regarding witchcraft prevail. Witchcraft is still used to settle interpersonal disputes among the Round Lake Ojibwa.

> A man possessing 'evil power' can be hired to harm someone you dislike. M., when he wishes to trouble someone, goes to North Spirit Lake and gets a man there to act for him.
>
> J. has 'evil power' or is thought to possess it. Also, A. and E.'s fathers possess the power. E. thought that S. might have power and also that N. should have power but perhaps not much and not used.
>
> This past spring E. woke up one morning and found one of his fingers was 'broken.' It apparently is alright now. The 'broken' finger E. believes is caused by someone trying to harm him with 'evil power.'
>
> A. went to North Spirit to preach and was threatened. A man told A. that he could harm him right there or when he returned home. After A. returned home he woke up one night and saw a man standing by his bed. It was the 'evil power' sent against him. He immediately began to pray and soon the 'evil power' vanished. Again one night A. woke up and he saw a tremendous snake approaching. It was the 'evil power' coming to try and harm him again. Again he prayed to God and the snake soon disappeared without harming him.
>
> J. once allowed M. to borrow his new canoe. When M. returned it, J. did not dare ask M. for any pay since he was afraid M. might get someone to use 'evil powers' against him. (Rogers, field notes)

Yet this characterization of the Ojibwa was limited to those who inhabited the Boreal Forest. The Ojibwa who moved onto the Plains appear to have lost these traits.[2] As Howard has said:

> Even the psychology of the Bungi apparently has changed with the movement from a Woodland to a Plains environment. Anyone who is familiar with both Woodland and Plains Indians quickly notes the Plains-like outlook of the Bungi.
>
> The fear of 'Black Magic' and 'Bearwalk' witchcraft which casts a pall over many Ojibwa woodlands communities, making everyone suspicious of his neighbour and fearful of his magic power, is a matter of little moment to the Bungi. (1965: 3–4)

Apparently, an extreme development of witchcraft was not compatible with conditions of Plains existence, but within the Boreal Forest, witchcraft could be maintained among people who found it still possible to survive in small fragmented groups. Large cooperative endeavours, such as those needed for the chute and pound, were not vital to survival.

Conclusions

Both the Cree and the Ojibwa outwardly present a picture of 'individualism.' But this is only a surface feature. On the basis of the data given, one might say for the Cree and a few Ojibwa groups on the edge of the Laurentian Uplands of Ontario that their social organization (hunting group-band) is an adjustment to a harsh environment in which natural resources are extremely limited and in which the people must scatter widely and in small groups in order to survive. Any larger grouping simply could not have secured sufficient food to maintain itself.

On the other hand, Ojibwa social organization (hunting group-band) which is so similar in most all respects to that of the Cree is not an adjustment to environmental restrictions and limitations, but rather a result of their preoccupation with, and highly developed concepts of, witchcraft in interpersonal relations in which no one can trust anyone else and there is constant fear of one's neighbours.

NOTES

1. I am indebted to Dr. David Damas, who organized the conference and who encouraged me to undertake the writing of this paper. Also I wish to thank my wife and the Director of the Royal Ontario Museum, University of Toronto, for their critical reading of the paper and Mrs. Mary Yeigh for making the maps and graph.
2. It might be argued that witchcraft developed late among the Ojibwa and had not yet diffused to the Plains Ojibwa or Eastern Cree. But Henry the Elder, in 1763, reports this custom most likely for the Ojibwa (1901: 117–8) who were only beginning to move permanently out onto the Plains at this time (Howard 1965: 12).

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ENVIRONMENT, HISTORY, AND CENTRAL ESKIMO SOCIETY[1]

BY

DAVID DAMAS

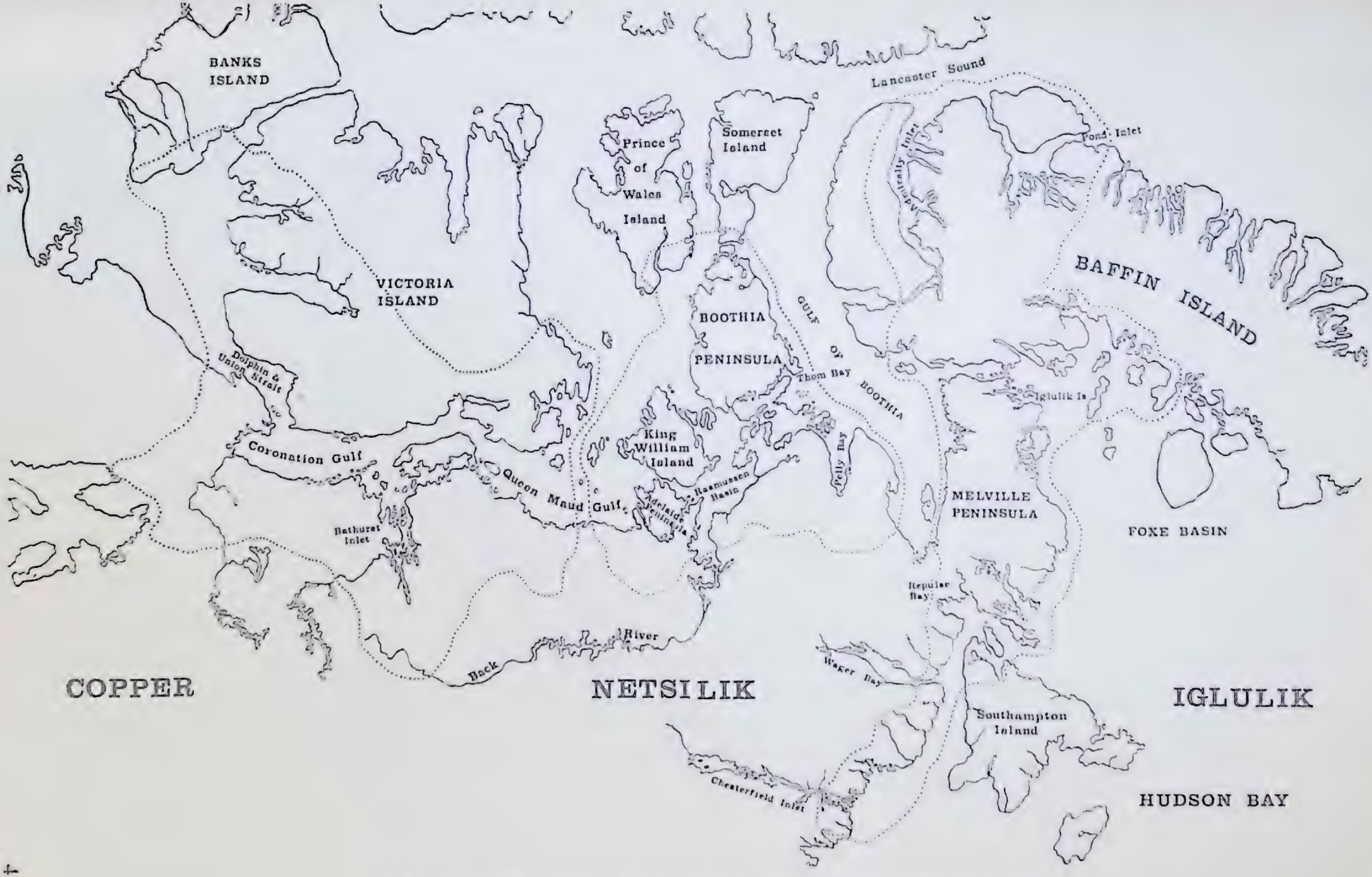
The social anthropologist's chief concern with cultural ecology concentrates on the possibility of finding determining or, at least, conditioning influences of environment on society. Helm (1962: 633) suggests that "it is not the gross categories 'environment' and 'technology' *per se* that are often critical in ecological inquiry, but rather the significant reticulation composed from them—the exploitative pattern." Likewise, Steward's (1955: 37) concept of the *culture core* is conceived of as the "constellation of features which are most closely related to subsistence activities and economic arrangements." Included in Steward's culture core are features of social organization, the chief subject matter of the social anthropologist's study.

One means of discovering possible relationships between social features and environment, or rather, exploitative pattern, is through an application of the "method of controlled comparison" (Eggan 1954). If one has for the region of his study an area within which there is a general homogeneity of culture and possibly a uniformity of linguistics and physical type, ecological factors can be used in two ways within a close comparative framework. First, if the ecology is also uniform within the area, an additional constant is provided as background against which the degree of uniformity or variability in social features can be assessed. If, on the other hand, the ecology varies within the area, one can posit the ecology as being a causal factor underlying possible variability in social features.

In this paper I will use material from the three contiguous Central Eskimo societies of the Iglulik, Netsilik, and Copper Eskimo to explore problems of the relationships of ecological factors (the exploitative pattern) to social features. That the Eskimo area is one which should provide a good setting for the method of controlled comparison can be seen from a brief examination of linguistic, physical anthropological, ethnographic, and archaeological material. Linguists (Swadesh 1951; Hirsch 1954: 834) have indicated that all Eskimo dialects north and east of Bering Straits (including those of the three groups treated here) show enough affinity to be regarded as one language. Oschinsky (1962) has posited the arctic Mongoloid racial type which shows a high degree of uniformity in physical features across arctic America and Asia, as contrasted to populations of subarctic America, temperate and tropical America and Asia.

Comparisons of oral tradition (Rasmussen 1931; 1932) and material culture (Birket-Smith 1945; Mathiassen 1928) show a large body of shared elements for the three central groups considered here. In addition, archaeologists (Meldgaard 1960: 60; VanStone 1962: 35; Taylor 1966: 117) indicate that the peoples inhabiting the central regions treated in this paper probably stemmed from a common ancestry, the carriers of the Thule culture.

This background of uniformity provides a base for a number of comparisons, some of which will be treated in this paper. The procedure of presentation will be the following: (1) The important features of the exploitative pattern—



Map 1—Copper, Netsilik, and Iglulik Eskimo: approximate aboriginal ranges

habitat, resources, technology, and seasonal cycles of economic activities—will be outlined for the three groups. (2) Descriptions of main features of settlement and social organization will be given. (3) An attempt will be made to isolate those social features which can be understood in terms of adaptation to the exploitative pattern. (4) Factors outside this adaptive nexus will be examined in light of their being possible determinants of other social features.

Habitat and Resources

The regions that encompassed the aboriginal[2] ranges of the Copper, Netsilik, and Iglulik Eskimo lie largely within the Arctic continental climatic zone of the northern Canadian mainland, with the edges of the area extending into the high Arctic zone of the Arctic Archipelago. Climate is characterized by long, cold winters and short, cool summers. Mean temperatures within these regions vary for the coldest month, February, from −20° F to −32° F and for the warmest month, July, between 42° F and 49° F. The area is not one of high winds, but absolute calm is rare. Precipitation is low, but relative humidity is quite high (Arctic Pilot 1959: 121–31; 1959*a*: 405–6; 1961: 346–7; Thomas 1961: 942–55).

Dates for the breakup of sea-ice vary from early or mid July in the extreme west to late July or early August in the central and eastern parts of the area. Freezing of the sea commences generally in late September or in early October. The period when the sea is mostly clear of ice lasts for less than two to about three months, although some of the principal gulfs are not frozen over until some time in November. For the remainder of the year, landfast ice welds most of the area together virtually into a continental mass. Areas of moving ice or open water are found each winter in parts of Foxe Basin, northwestern Hudson Bay, and Lancaster Sound, all bordering on Iglulik Eskimo country. The Gulf of Boothia, on the eastern boundary of the Netsilik region, usually has large expanses of moving ice and open water along a floe edge during much of the winter (Arctic Pilot 1959: 107–21; Balikci 1964: 55).

The surface of the land alternates from tundra to gravel and exposed rock. Some products of the earth had considerable value for the inhabitants. For instance, in all three regions they used steatite for lamps and pots, and the Copper Eskimo used float copper to make a number of implements. The general absence of wood placed a severe limitation on the material culture of the three groups, although the Copper Eskimo were more fortunate in this respect in that they could reach wooded areas. Also the shores of their land were better endowed with driftwood than were those of the groups to the east.

Willow, which grew in a number of places in the three regions, was at times burned as fuel (as was *Cassiope tetragona*) or used for sleeping mats. Berries were picked in season. On the whole, however, the occurrence of flora in the area was important because of its influence on the distribution of game animals, especially the caribou.

Caribou were found in the three regions both in resident winter herds and in migratory herds that wintered in the taiga to the south but crossed to the islands and peninsulas of the area in spring, returning south again in late autumn. The poor vegetation base kept the resident herds at a minimum on Banks, Victoria, and King William islands and on Boothia and Melville peninsulas, but Baffin Island supported extensive herds the year round

(Macpherson n.d.; 1961; Manning and Macpherson 1958: 65–7). Winter herds seem to have been restricted in range to highland areas where snow was less deep and vegetation was plentiful enough to support the animals.

Fish (principally arctic char, which spend five or six weeks of the year in salt water, and lake trout) were apparently widely distributed throughout the lakes and streams of the area. Tomcod were also used for food, though they generally were considered an emergency ration.

The most important sea mammal in the area was the ringed seal. The distribution of that animal or its availability as game was not influenced by food supply alone. Rather, the condition of the sea-ice was of vital importance (MacLaren 1962: 169). The ringed seal is found either in open water or in areas of new ice since it is impossible for the animal to keep breathing holes open in ice of the thickness formed over a two-year period. Unadult animals appear to range outside the area of landfast ice apparently because of their inexperience or inability in keeping breathing holes open (MacLaren 1962: 171). My personal observations indicated that breathing holes are most likely to be found along fissures. Thus areas where there has been action of wind and current causing fracturing are generally good sealing grounds in winter.

Other sea mammals were the bearded seal (an animal which is three or four times as large (600–800 lb) as the ringed seal), the walrus, the narwhal, and the beluga. Among these larger animals only the bearded seal was found in significant numbers in the other two areas, and only in the Iglulik Eskimo regions were any of them hunted extensively. Polar bear were encountered in various localities in the other two areas, chiefly in places favourable to denning or where there was rough ice, which enabled them to stalk the seal with reasonable prospects of success.

Muskoxen could be found scattered in a number of places in the regions (Anderson 1930) but comprised a principal source of food only around Wager Bay (Boas 1888: 449–50) in the Iglulik Eskimo region and at Bathurst Inlet and northwestern Victoria Island (Jenness 1922: 105) in Copper Eskimo regions.

Technology of Exploitation

Implements used in harvesting faunal resources form an important part of the total exploitative pattern. Members of the Fifth Thule Expedition of 1921–24 (Mathiassen 1928; Rasmussen 1931, 1932; Birket-Smith 1945) have produced detailed inventories of the material culture of the three regions. Birket-Smith's (1945: 220–5) comparison of implements used in the major economic pursuits shows that of twenty-five artifacts used in caribou hunting eight occur in all three regions, six in both Copper and Netsilik regions, four in Netsilik and Iglulik regions, one in Copper and Iglulik regions, and the remaining six in one of the three regions only. Of seventeen seal hunting artifacts, nine were used in all three regions, two in Copper and Netsilik regions, three in Copper only, and three in Netsilik only. Of the twelve fishing artifacts that were known, seven were used in all three regions. Three of four chief fowling implements were used in all three regions, one being absent in the Iglulik region only.

It is evident that a large number of hunting implement types are common throughout the area. This is not surprising when, on the one hand, many exploitative techniques are identical and when, on the other, the general material culture of the three regions shows strong evidence of shared tradition.

It should be noted, however, that Birket-Smith's listing does not include artifacts such as special harpoons or sea-going kayaks, which are associated exclusively with the hunting of large sea mammals. These artifacts were found in the Iglulik Eskimo regions and not among the more westerly groups considered here.

Among hunting peoples, the Eskimo has frequently been considered a leading example of a group having a highly developed material culture. This designation seems justified when one examines the substantial inventory of specialized hunting tools found in the central area alone. This inventory of usable hunting tools is especially remarkable when one considers that raw materials for construction, especially the all-important wood, were often hard to acquire.

Seasonal Economic Cycles

The pattern of exploitative activities over the year's cycle for each of the three groups is shown in Table 1. It can be noted that the seasonal cycles of the Copper and Netsilik Eskimo were almost identical. During the winter, breathing-hole sealing (*mauliqtuq*) was the principal economic activity, beginning in December or in January and extending well into May. After that, *uuttuq*, or basking seals, were occasionally hunted in parts of the regions. In general, however, this hunting technique was not well developed in the Copper Eskimo or Netsilik regions.[3] Generally a period of tomcod jigging followed and that was succeeded by the phase in the cycle when fishing through lake ice was the primary pursuit. In most places, caribou hunting was a secondary occupation during the spring and early summer, because at that time the caribou were lean and the skin was in poor condition. The period of real usefulness began in August when back fat had begun to form and the new fur provided excellent inner garments. At the end of August the important run of the arctic char back into the rivers drew the Eskimo to places where *haputit*, or stone weirs, had been built. Considerable numbers of char were speared with leisters at these places.

Caribou hunting, which began in earnest in August and extended well into the autumn, was conducted mainly in two ways. Drives were organized so that women and children channelled the animals through rows of rocks, arranged in such a way as to resemble men (*inukhuit*), toward bowmen or lancers who waited at points of convergence in firing pits. The second method was the use of kayaks at crossing places where the swimming deer were lanced by kayakers.

A period of relative idleness ensued in late fall during which a few trout were jigged through lake ice, but the principal activities were the sewing of winter clothing by the women and visits to caches of meat and fish by the men. After the clothing was sewn, and generally when the stores of food from the summer had run low, the groups moved back to the sea-ice to commence winter sealing.

The cycles shown on the chart are more or less typical for the two regions, but some variation did occur. For instance, for the Copper Eskimo who summered on Victoria Island, fish were probably more important than caribou, the reverse being true for other Copper Eskimo groups such as the Ahiarmiut of Queen Maud Gulf and the Bathurst Inlet group. The Copper Eskimo who wintered off the southeast coast of Banks Island hunted bear extensively enough so that that animal may have played as important a role in their winter life as

TABLE 1 – SEASONAL CYCLES

	JAN.	FEB.	MAR.	APR.	MAY	JUN.	JUL.	AUG.	SEPT.	OCT.	NOV.	DEC.
COPPER	polar bear					musk ox hunting						polar bear
						tomcod	lake trout	char	secondary fishing			
					secondary caribou hunting			principal caribou hunting				
	mauliqtuq sealing				uuttuq							mauliqtuq
		live on sea ice live in large aggregations (av. 100)				nomadic life on land scattered groups of 3-20		groups to 50 at fish runs		live in larger aggregations 30-100 at points of land		
NETSILIK						tomcod	lake trout	char	secondary fishing			
					secondary caribou hunting			principal caribou hunting				
	mauliqtuq sealing				uuttuq							mauliqtuq
		live on sea ice live in large aggregations (av. 100)				nomadic life on land scattered groups of 15-30		groups to 50 at fish runs		live in larger aggregations 25-100 at points of land		
IGLULIK						occasional musk ox hunting						
					occasional char fishing							
				secondary caribou hunting			principal caribou hunting					
	secondary walrus hunting						walrus hunting from kayaks					
				narwhal hunting			narwhal hunting from kayaks					
	mauliqtuq sealing				uuttuq sealing		sealing from kayaks					
		live in large aggregations (av. 100) on sea ice (also smaller groups 15-30)			small groups on on sea ice (av. 20)		small groups at points of land (av. 20)		older men at points of land; younger men inland		aggregations at points of land (up to 100)	

[Copper and Iglulik cycles adopted from Weyer (1932)] did the ringed seal (Stefansson 1914: 30; Jenness 1922: 100).

In the Netsilik regions the Iluilirmiut of Adelaide Peninsula provided an almost unique example of extensive char fishing through the sea-ice. They camped as a group each summer in July at Richardson Point where the char were speared through holes in the ice as they passed on their way seaward from the mouth of the Back River. The Netsilik of Thom Bay continued into June to hunt seals at their breathing-holes because of the special hunting conditions and hunting techniques that prevailed there. The Pelly Bay Netsilik appear to have hunted the *uuttuq* to a greater extent than did other Netsilik (Balikci 1964: 42).

The section of Table 1 that refers to the Iglulik Eskimo is, again, a composite picture, an attempt to represent the seasonal cycles of the principal Iglulik Eskimo regions. For the southerly Aivilingmiut (Chesterfield Inlet to Wager Bay) and the Iglulingmiut (northern Foxe Basin) walrus hunting in the young ice or open water is equal in importance to *mauliqpuq* sealing. The main group of Aivilingmiut appears to have restricted winter activities to breathing-hole sealing. Also caribou hunting seems to have been more extensive and more successful in that region than in other parts of the Iglulik Eskimo area. Narwhal hunting, especially in spring, was of prime importance to the Pond Inlet and Admirality Inlet people. The animal was also hunted in spring from floe edges and in summer from kayaks. Whales were occasionally hunted from kayaks around Repulse Bay, and beluga could be encountered throughout the waters of Iglulik Eskimo regions. Muskoxen were apparently very important to the economy of the groups around Chesterfield Inlet and Wager Bay (Boas 1888: 449–50).

Local variations, which have been discussed for the three major regions, could obscure the generalized picture of contrast between the cycles of the Netsilik and Copper Eskimo on the one hand, and the Iglulik Eskimo on the other. It is in the much greater emphasis on the sea-mammal phase of subsistence that the yearly cycle of the Iglulik as a whole contrasts to those of the other two major groups. This increased importance of sea animals was expressed in several ways: first, the higher development of *uuttuq* sealing, its universality in the area, and the long periods of the cycle during which this practice was carried out; second, the practice of hunting larger sea mammals from floe edges; third, the hunting of seal, narwhal, beluga, walrus, and even occasionally whales from sea-going kayaks.

Exploitative Zones

When contrasted to exploitative patterns that occur in other areas outside the Central Eskimo regions, the variations in exploitative pattern found among the three regions covered here do not seem to be great, but since I am attempting to ascertain ecological microvariations, it may be justifiable to consider the area encompassing the three groups as being divided into two exploitative zones. The western zone comprises the regions stretching from southern Banks Island across western and southern Victoria Island, Coronation and Queen Maud gulfs, King William Island, Boothia Peninsula to Pelly Bay — the regions of the Copper and Netsilik Eskimo. The eastern zone extends from around Chesterfield Inlet, north along the northwestern shore of Hudson Bay and the western and northern shores of Foxe Basin to across the northern third of Baffin Island — the country of the Iglulik Eskimo.

These zones are not set off sharply by physiographic boundaries. There is high and low land in each zone. The conditions of the sea-ice do, however, differ because larger areas of permanent open water with drifting ice are found in the Iglulik Eskimo country. The division into two zones is based on the total exploitative pattern: distribution of fauna, differences in technology, and differences in the seasonal cycle of exploitation. Briefly, to summarize the above discussion, the western exploitative zone can be characterized as a region within which exploitation was sharply divided into land and sea hunting phases, whereas in the eastern zone the sea-hunting occupations were carried on over a much greater part of the cycle, overlapping caribou hunting and replacing lake fishing to a large extent.

Dispersal of Personnel

In addition to the outline of the seasonal economic cycles, the variations in group size over the year's cycle are shown in Table 1. The largest aggregations occurred at the sealing villages. These groupings endured for periods of two to five months. At times, as for instance in the Dolphin and Union Straits area (Jenness 1922: 120), the groups that gathered in the late fall while the winter clothes were being sewn were separate for the early part of the sealing season but merged with at least one other such group to form larger assemblages. Elsewhere, especially in much of the Iglulik area, the sewing group was often coterminous with the winter aggregation so that the actual duration of the group could have been as long as six months, even though the site of settlement would be moved several times during that period, and a certain amount of fluctuation in membership took place.

In the Netsilik and Copper Eskimo regions fifty or more people aggregated for brief periods at the sites of fish runs. These assemblages probably endured for a period of one to three weeks.

During most of the remainder of the year, groups were fragmented. In the Copper Eskimo area, group size was variable. Occasionally, the nuclear family might comprise a detached group for a period of a week or two (Jenness 1922: 130, 134, 137, 139). Jenness's (1922: 121–44) description of the fluctuating size of the hunting group which he accompanied for seven months in 1915 seems to agree with statements from informants living farther east in the area. Fluctuation in size from the nuclear family of three to five to a maximum of about twenty appears to have been typical for the season from about May to November, although occasionally in parts of the region aggregations of fifty or more seemed to have occurred briefly, as noted above, usually in conjunction with fish runs or possibly with caribou drives as well. In the Netsilik area groups of fifteen or twenty seemed to have remained intact during the major part of the spring, summer, and autumn, except when joining with one or two similar groups at sites of fish runs.

In the Iglulik regions the winter aggregations began to split in May when the *uuttuq* season commenced. Spring and summer groups of about twenty appear to have been fairly typical. Later, in the late summer and early autumn, the older men stayed at the coast and hunted in pairs or groups of threes while the youths moved inland for caribou (Mathiassen 1928: 24, 30).

Social Organization

Table 2 shows the distribution of several features of social organization in the three regions.

Table 2—FEATURES OF SOCIAL ORGANIZATION

Features		Copper	Netsilik	Iglulik
Group composition	Winter	Bilateral with prominence of male relevant ties	Bilateral with prominence of male relevant ties	Bilateral with prominence of male relevant ties
	Summer	Variable: kinship and partnerships	Kinship based, viri-oriented	Kinship based, viri-oriented
Household organization		Nuclear	Extended (viri-oriented)	Extended (viri-oriented)
Leadership		Ephemeral	*Ihumataq* heads extended family	*Isumataq* heads extended family. Also local group *isumataq*.
Basis of sharing practices		Partnerships	Partnerships and kinship	Kinship and local group sharing regulated by *isumataq*
Commensural units		Village	(1) Men of extended family (2) Women of extended family	Variable: (1) Individual (2) Nuclear family (3) Extended family
Kinship behavioural directives		Narrow in scope	Dominance hierarchy and restricted to extended family	Broad in scope Dominance hierarchy Closeness–distance
Marriage practices		Kin agamous[4]	Kin agamous	Kin exogamous
Uncle–aunt terms		Affinal including	Affinal including	Affinal excluding
Cross-sex cousin terms		Eskimo	Hawaiian	Hawaiian

Genealogical reconstructions of censuses (Mathiassen 1928: 15–20; Rasmussen 1931: 85–90, 1932: 78–85) from the early 1920's show that each winter grouping was made up of personnel who formed a continuous or near-continuous series of primary kinship ties if their relationships are plotted on a chart. Together these ties can be thought of as bilateral, but analysis demonstrates that there is a pronounced slant toward predominance of male-oriented links over female-oriented links between nuclear families (Helm 1969).

The summer groups of the Copper Eskimo were variable in composition with no evident great regularity of kin ties. This is contrasted to the pronounced continuity of kin ties and viri-orientation[5] of the Netsilik and Iglulik summer groups.

The nuclear family formed the Copper Eskimo household, whereas in the other areas extended families formed the chief household unit.

A pronounced egalitarianism characterized interpersonal relations among the Copper Eskimo. The Netsilik extended family was headed by a male, usually the oldest in the group. He was designated *ihumataq*. No overall local group leadership was known. In the Iglulik regions family organization was much the same, but each local group was ideally headed by an influential man who was also called *isumataq*.[6]

Food sharing in the Copper Eskimo area emphasized a system of partnerships based on reciprocal sharing of named parts of the seal. Otherwise there was no structured food division beyond the nuclear family.

The Netsilik had a similar system of sharing, but the food so procured was eventually the property of the extended family, which was the actual minimal unit of sharing.

The extended family also formed the minimal group for sharing among the Iglulik, but the local *isumataq* regulated the food division among extended families.

The chart shows regional differences in commensal practices.

Norms of kinship behaviour were not highly developed among the Copper Eskimo. At the other extreme each Iglulik Eskimo was governed by directives for behaviour based on closeness–distance and dominance–subordinance axes (Damas 1963: 48–51; 1964) that extended as far as kinship was reckoned. In the Netsilik area the universe of behavioral directives does not seem to have extended much beyond the confines of the extended family.

Variations in marriage practices and kinship terminology are noted in Table 2.

In addition to those features shown on the chart, a body of widespread Eskimo social customs such as spouse exchange, adoption, child betrothal, associations in dancing, joking partnerships, and other voluntary or quasi-kinship relationships existed throughout the area, though at times these institutions were expressed in locally variant ways.

If the societies of each of the three major groups are considered separately, good arguments could be made for conceiving of each as a fairly well integrated or internally consistent arrangement of working parts. For the Copper Eskimo there is consistency in the subordination of kinship factors — the narrow scope of behavioral directives, the use of names rather than kin terms, the absence of kinship obligations in sharing beyond the nuclear family, and the relative independence of the nuclear family (with regard to household organization and sharing). Altogether there is a disregard for kinship factors that is remarkable in a hunting society.

Compensation for this lack of emphasis on kinship can be found in the importance of various voluntary or quasi-kinship relationships. Spouse exchange, joking, dancing, and seal sharing were the main situations in which partnerships were developed or expressed. The communal commensal practices can also be seen as balancing the lack of compulsory sharing outside the system of partnerships and outside the elementary family. On the other hand kinship appears to have played an important part in marriage arrangement, adoption, and in hard-to-objectify areas of affectional life.

In the Netsilik area kinship was more important. There was a hierarchy of authority within the extended family which operated according to seniority in the male line. The extended family was a cohesive household and sharing unit. It was the local group during much of the year's cycle. Mutual aid is achieved largely within that unit. Its members were recruited strictly on the basis of kinship. On the other hand, the seal-sharing partnerships, spouse-exchange partnerships, and other voluntary associations served to span the otherwise sharp boundaries that existed between extended families.

For the Iglulik Eskimo, the kinship system and the effective social structure appear to have been much more coterminous. The range of kinship behavioral directives was expanded beyond the extended family and operated on a wider scope as an effective regulator of interpersonal behaviour. The importance of kinship is further emphasized in the selection of the settlement leader. The *isumataq* of the largest kin group in the local settlement was usually designated as *isumataq* of the whole group (Damas 1963: 196). Voluntary sharing partnerships were not present. Food distribution was also regulated by the village *isumataq* as noted above.

To be sure, one could conceive of inconsistencies within each of the three social systems. For instance, the practice of cousin marriage among the Netsilik is inconsistent with the Hawaiian type cross-sex-cousin terminology, and the broad extension of cousin reckoning among the Copper Eskimo appears to run counter to the narrow scope of behavioral directives and the general weakness of Copper Eskimo kin ties. It should also be noted that all the voluntary relationships mentioned above, with the exception of the seal-sharing partnerships, existed in Iglulik Eskimo regions and provided a secondary system operating alongside the kinship system, which I have characterized as providing the major force in integration. We cannot, in other words, demonstrate the existence of an equalizing mechanism that would ensure equivalence of institutions either in number or effectiveness among the three societies.

This attempt to portray each of the three societies as representing alternative but "internally adaptive" (Eggan 1955: 82) or integrated wholes must also be tempered by an appreciation of the limits of our knowledge. Gaps in the ethnographic record for the period of early contact, the disappearance or modification of social features at the time of my field-work, and the pitfalls of historical reconstruction carried out through statements of informants all provide possibilities for erroneous interpretation. Nevertheless, on the basis of the information that is available, a fairly good case can be made for the "concomitant variation" (Nadel 1951) of many social features among the Copper, Netsilik, and Iglulik Eskimo.

The comparative method can thus help indicate alternative ways of achieving internal adaptation in the three societies. It can also help to explicate ways that the societies adapt to the external system of the exploitative pattern.

Adaptation and Central Eskimo Society

If major features of society are thought to be related to major aspects of the exploitative pattern, the following associations can be expected to occur: (1) in regions of uniform exploitation, social features should also show a high order of uniformity; (2) in regions encompassing variations in exploitation, these variations could be expected to be accompanied by certain parallel variations in social features; (3) in order that the associations of specific social features can be convincingly related functionally to specific exploitative features, certain logical requirements must be met. It does not, for instance, appear fruitful or valid to match kinship terminologies of various Eskimo groups with the gross habitat areas where they occur. Rather, factors that have immediate relevance in a chain of associations must be sought.

A number of the elements of the exploitative pattern were found to be held in common in the regions of the three major groups. Occurring congruently with these features of exploitation is a body of shared traits related to dispersal of personnel. Arguments, can, I believe, be advanced to support the contention that some of these associations are not spurious.

The aggregation of people into large winter groupings can be argued as being strongly related to the expediencies of *mauliqtuq* sealing. The number of breathing-holes kept open by individual seals is not known. Testimony of Eskimo from the three regions, does, however, indicate that the lone hunter or even several hunters have little chance of success. A large number of hunters offers great advantages in this type of hunting. The typical winter grouping of over 50 and perhaps up to 150, or occasionally even 200, persons appears to be a unit well adapted to breathing-hole sealing over the area of a circle which has a radius, from camp, of about five miles. That was the range (Stefansson 1913: 169–70) that would generally be exploited and that appears to be related to the limit of a normal day's activities including the walks to and from the sealing grounds and the camp.

Smaller winter groupings did occur at times in part of the area, but it seems doubtful that in most parts of the area optimum exploitation of seals through standard *mauliqtuq* procedures was possible for groups under fifty (representing about a dozen hunters). In the Netsilik regions a group ranging in size from fifteen to about thirty persons lived at Bellot Strait during the 1920's and 1930's but women and children had to be enlisted to aid the winter sealing at that place. Also during that period the use of the rifle for summer hunting of sea mammals as well as caribou enabled the people from that locality to accumulate large stores of meat for the winter. In the Iglulik area (Damas 1963: 59–66) local groupings of ten to thirty appear to have existed apart during the winter, perhaps even before the use of the rifle altered subsistence procedures to any appreciable extent. In these cases hunting of larger sea mammals, especially the walrus, over a large part of the year's cycle must have taken some of the pressures from *mauliqtuq* sealing.

It seems evident, then, that the occurrence of large winter aggregations has a strongly deterministic base in the method of sealing that was generally the sole reliable means of livelihood at that season. On the other hand, social motives for aggregation are also apparent. The winter assemblages provided opportunity for reaffirming social ties that had not been realized during much of the preceding phases of the year's cycle. It was at these winter gatherings that chief occasions for shamanistic performances, games, and dances took place. We hypothesize that even if the necessities of *mauliqtuq* sealing had not been pressing, we might expect aggregations to form if game conditions allowed, in order that these social events could take place. However, the long duration of these aggregations and their occurrence in winter probably can be more closely related to economic necessity than to social advantages.

Fragmentation during much of the remainder of the year's cycle bears relation in a general way to the mode of hunting and the dispersal of game. Lake fishing and the hunting of scattered groups of caribou, small game, and fowl could, under most circumstances, be most profitably carried out by small groups spread out over the land. The minimum size of the summer groupings was roughly influenced by these factors. On the other hand, the tendency of Copper Eskimo hunting groups to split occasionally to the level of the nuclear family (Jenness 1922: 127–42), compared to larger minimum groupings found in the other two areas, appears to be more closely related to the differences in family organization in the two areas than to economic factors.

Larger aggregations in summer occurred at sites of weir fishing or occasionally at caribou-crossing places. Both of these activities could be carried out with a lesser number of persons than were actually present at these occasions, though it may be that the number of places available for intercepting caribou or char during their migrations was limited. The assemblages of fifty or more persons at those times were made possible by the concentration of game (caribou and fish), but it would be difficult to demonstrate that these aggregations represented an economic necessity of the urgency found in the assembling of people for the winter sealing period.

Older male Iglulik Eskimo generally hunted in kayaks in two's or three's; the advantage of this teaming is especially evident in the pursuit of the larger sea mammals. Numbers larger than two or three, however, do not appear to have been necessary for these activities.

Among the various kinds of groupings that formed over the year's cycle, the autumn aggregations at the sewing places probably related least to economic necessity. Economic activities were virtually at a standstill in that period and cannot be thought of as providing crucial factors in bringing about the aggregations at that time. Rather, it is easier to imagine that the attraction of extending contacts outside the small groupings that had been the local unit during most of the preceding phase of the cycle was the chief motive for joining into larger groups at that period.

Chang (1962: 28) has designated the spatial and temporal aspects of groupings as "settlement pattern" and finds its determinants to be related to "cultural ecological forces" as opposed to "community pattern" which is relatable to "efficient causes in the sphere of sociology and social psychology." While I have indicated that adaptive factors of the exploitative pattern strongly influence settlement pattern, I also consider sociological or social psychological factors to be important on that level.

If attention is directed toward features subsumed under social organization (in Chang's terms "community pattern"), it is apparent that a number of these features are uniform in character throughout the entire area. It might be productive to examine them from the point of view of their representing adjustments to features of the exploitative pattern which are held in common.

The composition of the largest aggregations was uniform in the three areas. An expansive network of primary and secondary kin ties, which evidence a slant toward dominance of male ties, can perhaps be related in a general way to classic expectations for hunting societies; that is, the greater importance of the male in the economy accounts for dominance of residential alliances (at the level of the large aggregation) based on male ties. Demographic factors would tend to limit the dominance of these ties and inject considerable bilaterality into group structure. However, if these characteristics of group structure are related to generalized adaptive features, they do not explain such situations as the apparent preference for matrilocality among the Nunamiut Eskimo of Alaska (Posposil 1964; Gubser 1966), also a hunting people. Extending the scope of comparisons a little wider, Helm's analysis (1965, in press) of Déné group structure shows a much more even balance between male-oriented and female-oriented linkages. These differences pose questions of comparison that are beyond the scope of this paper but leave the question of adaptiveness of Central Eskimo group composition unresolved until further detailed information is available from other Eskimo regions.

Other social features, which are general within the three regions dealt with here, are institutionalized adoption, child betrothal, and spouse exchange. All of these, it could be argued, provide social mechanisms that are well adapted to external conditions.

Turning first to adoption, the "demographic hypothesis" (Dunning 1964) relates to redistribution of personnel among families to control its size to within a range suitable for the survival conditions of the Eskimo. Indeed, this hypothesis could be carried further to include redistribution according to sex as well as number. This is one of several important functions of adoption, one of the others being grandparent–grandchild adoption, or the forming of new nuclear families for aged couples after their offspring have left home. Both of these chief patterns of adoption would appear to be well adapted to extreme conditions encountered in the Central Eskimo area. In order to fortify this association, however, it would be necessary to examine adoption data from a number of Eskimo societies to determine whether adoption is more frequent or more highly structured in marginal subsistence areas, compared to better-endowed areas.

Child betrothal also appears to have survival value in the area. In the Netsilik and Copper Eskimo regions, where female infanticide was in force, assurance of securing a mate was quite important for the young male. By arranging marriages early in infancy, males could be assured a fair likelihood (barring childhood death of the betrothed) of getting a mate. The problem of sex imbalance did not occur in the Iglulik area where selective female infanticide did not occur, but the practice of kin exogamy had the effect that with the small populations a young man had often to travel some distance to find a woman outside the range of ineligible spouses. Child betrothal would have merit under those circumstances as well. Before we accept child betrothal as being peculiarly adapted to special conditions of Central Eskimo life, it is well to note that, along with adoption, betrothal is known over a wide expanse of the Eskimo area. Here again, further comparative work would be required in order to learn whether variations in the practice throughout the Eskimo area can be associated with specific local conditions.

Spouse exchange has been discussed as a means of extending the kinship network (Spencer 1958; Guemple 1962; Heinrich 1963), and it is to be expected that factors of mutual aid and cooperation might also be extended in parallel fashion under circumstances of exchange. Expansion of the kinship network would indeed have advantages in areas like the Iglulik regions where kinship was the chief means of achieving integration and would be advantageous as well in the other two regions, even though long termed specific economic advantages may not have been implied. But spouse exchange has other functions which do not relate directly to ecology or to social structure. Among these may be tensions inherent in monogamous sexual life or the need for intimate comradeship perhaps not provided for in the kinship network.

Thus some of the traits which are shared within the three regions could be argued as having adaptive value for the area. At the same time the occurrence of most of them throughout much of the Eskimo area may indicate that their adaptive value is geared to a more generalized arctic environment or even perhaps in some cases to a more generalized hunting economy.

While certain general features of the environment and of exploitation are similar throughout the area covered here, I have posited two exploitative zones in terms of difference in fauna, technology, and seasonal economic cycles. If one looks to those zones it is possible to see that some social features are congruent in distribution to the two zones. Some of these may be logically defended as adaptive.

A hypothesis can be advanced that links a demographic fact, largely dependent upon exploitative efficiency, to a prominent social practice. I have noted the occurrence of marriages between relatives among the Netsilik and Copper Eskimo, a practice in contrast to the kin exogamy of the Iglulik Eskimo. In the former two areas female infanticide was practised and was quite clearly linked to survival levels (Rasmussen 1931: 84, 139–41; 1932: 70). This was not the case in the Iglulik area where Mathiassen (1928: 212) thought superior economic conditions obviated the practice of infanticide. In a society where the male to female ratio is three to two, the choice of mates is restricted for males, and probably crucially so, if the permissible marriage unit were to exclude kin. It would appear, therefore, that for the males to have a fair chance for choice of mates, there would have to be a relaxation of incest tabus. In the case of the Igluliks, however, who had a more evenly balanced sex ratio, it was possible for them to marry outside a reasonable degree of collaterality but still within the Iglulik Eskimo population of about 500 people.

To some extent these variant marriage practices are expressed in the arrangements of terms. The careful separation of consanguines and affines in most of the terminology of the Iglulik Eskimo (Damas 1963: 55) is consistent with the strict observance of marrying outside one's consanguineal group. The "affinal including systems" (Heinrich 1960) of the Netsilik and Copper Eskimo also appear to be products of marriages between relatives.

The presence of seal-sharing practices of the Copper and Netsilik Eskimo and their absence among the Iglulik Eskimo are another example of a parallel of exploitative zones and features of social organization. One could advance an argument that would link the two. With the lower level of subsistence or exploitative efficiency in the Copper and Netsilik regions, it can be seen that the highly structured systems of sharing could provide special insurance for unsuccessful hunters. By contrast, the lack of these partnerships in the Iglulik area could be interpreted as an expression of less urgent ecological pressures. However, it would be exceedingly difficult to demonstrate that the system of distribution based on kinship and authority networks would be a less efficient or a less valuable insurance than that of the voluntary partnerships. I will return to a consideration of these associations later in the paper, but at this point it should be noted that the occurrence of seal-sharing partnerships in their exploitative zone does not carry with it compelling logical association when close-ranged comparison is applied.

In addition to these social traits whose distribution appears to coincide with ecological factors, both those traits shared in the three areas under discussion and those shared within hypothesized exploitative zones, another set of social characteristics exists. This set includes those traits that were shared across boundaries of two zones but were not found within the third. Specifically, some social features were found to be identical for the Iglulik and Netsilik Eskimo but were different for the Copper Eskimo. Some of these features concern contrasting family structures. The occurrence of a group connected mainly through father–son and brother–brother ties as a unified residential cooperative and sharing unit in the Netsilik and Iglulik area contrasts sharply with the highly nucleated family structure of the Copper Eskimo area.

This situation is also revealed in terms of residence. In general anthropological works (Linton 1936: 163; Murdock 1949[7]: 228; Service 1958: 77) the Eskimo have been characterized as neolocal. This characterization appears to apply well to the Copper Eskimo if we think in terms of household organization, for each young man generally established a separate household. This is not true, however, of the Iglulik and Netsilik Eskimo for the majority of their residential situations were patrilocal, at least during the early years of marriage (except in cases of bride service).

The basic difference in the family organization of the three Eskimo groups is expressed in the contrast in organization of the hunting groups of the summer phase of the year's settlement cycle. In the Copper Eskimo area many nuclear families shifted associations from those based on close kin ties, for instance, father and son, to those based on spouse exchange, and to those based on unformalized friendships. Alternately one or the other of these associations might have occurred during the course of one season or from one season to the next. Informant testimony indicates that only the loosest regularity appears to have been present in the make-up of the Copper Eskimo hunting groups of the summer phase of the year's cycle.

In the Netsilik and Iglulik regions the core of each summer group was an extended family, though I have also noted that around Iglulik and Repulse Bay there was a horizontal splitting of these groups for a period in late summer and fall. Even then, however, though they were spatially segmented, the extended families maintained their unity as a sharing unit because when they recombined, the entire unit was the repository for the meat and skins taken in the combined land and sea hunts. The chief, and common ingredient of the summer hunting groups of the Iglulik and Netsilik Eskimo, then, which set them off from the hunting groups of the Copper Eskimo, was the close-knit kinship ties that existed in most or all of the former groups.

The tendency to split occasionally to the level of the nuclear family, found among the Copper Eskimo, is probably more an expression of the brittleness of ties outside that unit than it is an expression of economic expediency, that is, if we accept their sharing of a common exploitative zone with the Netsilik Eskimo. By contrast, the larger minimal groupings found in the Netsilik and the Iglulik areas express their similarity in family structure with strong solidarity of kin ties within the extended family.

Although the character of kin-based linkages in the maximum aggregations appears to be identical in the three regions when portrayed on charts of kinship composition, the actual segmentation of the winter groupings differs. Whereas the Netsilik and Iglulik Eskimo groupings were divided into extended families as household and minimal sharing units, the Copper Eskimo winter aggregations can be conceived of, operationally, as clusters of nuclear families.

If one seeks to relate this marked contrast in family organization together with its concomitant effects on group organization to the factors of exploitation, it would be difficult to find much association between the two levels. There does not appear to be a strong relationship between the type of cooperative activity and the size and character of family units. The chief activities which require more than one or two men are *mauliqtuq* sealing, hunting caribou at drives, and possibly fishing at weirs, though all of these activities could be carried out when necessary with the aid of women and children. All three of these subsistence activities are known in each of the three regions (though fishing was not an important Iglulik Eskimo occupation) and thus do not parallel in their distribution the distribution of family type.

The activities that set off the Iglulik region as an exploitative zone, namely, extensive *uuttuq* sealing, floe edge hunting of walrus or narwhal, and kayak hunting of sea mammals, can all be carried out by a lone man or preferably by pairs of men. In all cases of cooperation in any of the areas there is no real necessity for the work force to be recruited from closely related personnel such as make up an extended family household.

Another feature that is shared across the ecological boundary between the Netsilik and Iglulik regions is the pattern of classing cross-sex cousins as siblings (Hawaiian type). This classification contrasts sharply the practice of separating cousins and siblings in the Copper Eskimo area.

In seeking to understand distributions of terminological features it is necessary to consider the distribution of structural features that may influence them. In the case of the Iglulik Eskimo, classification of cross-sex cousins with siblings is consistent with the exogamous marriage practices, according to accepted theory. In the Netsilik example, however, one marries one's classificatory siblings. Thus the distribution of this terminological trait does not conform to the distribution of expectable concomitant social practices, so that the possibilities of relating this distribution to exploitative practices through the social feature do not appear to be promising.

There is still to be considered a fourth category of social traits, those which appear to be expressed with a fair degree of individuality in each of the three regions.

While some factors of the distributive network, such as the basic framework of Copper and Netsilik Eskimo seal-sharing partnerships, are held in common by two or more of the groups, the total distributive networks, including commensal practices, emerge as three essentially separate systems. Though I have described variations in sharing and commensal practices as they relate inwardly in a consistent manner in each of the three social systems, their distribution cannot be correlated to a distribution of exploitative practices, for again there are three systems in two exploitative zones.

Another example of separation of traits in each of three regions is the patterns of leadership. It has been noted that the level of leadership was village-wide in the case of the Iglulik Eskimo; in the case of the Netsilik it did not extend beyond the extended family; and in the case of the Copper Eskimo, egalitarianism prevailed. Each of these variants has been presented as paralleling the effective range of the kinship system of each of the major groups and, in effect, is being presented as another aspect of the distribution of kinship systems. Either as separate systems or as part of the kinship system, the distributive networks again do not correlate in geographical distribution with exploitative zones.

A third feature that is split in its distribution between the three regions is the classification of parallel sex cousins. The Copper Eskimo have a two-cousin system, the Netsilik a one-cousin system, the Iglulik a three-cousin system.[8] While I have argued (1964) for an understanding of the arrangement of Iglulik cousin terms according to the principle of the solidarity of the sexes operating in conjunction with a system of closeness and distance among classes of relatives, no such explanation was forthcoming from my examination of the Netsilik and Copper Eskimo terminologies. Whatever the structural features to which the classification of parallel sex cousins may relate, neither they nor the terminology itself can readily be interpreted as correlating with ecological factors because two systems exist within one exploitative zone.

To the thorough-going ecologically oriented field-worker, my attempts to use ecology as a control will seem superficial. The dedicated ecologist would be concerned with an accurate count of game in the area, with calorific intake and output, etc. These approaches are not possible for the areas I have studied, for the Central Eskimo area presents changed cultures and societies operating in changing ecological regions. It appears to be within the realm of possibility that if such a study had been made at an aboriginal period it might have provided material to support the argument that three rather than two ecological zones (coinciding with the habitats of the three groups) could be posited, and thus the zones would correspond to the distribution of the features that are individual to each of the areas. If that were indeed the case and, if in addition, arguments could be advanced for linking the three zones with social features that similarly diverge, the distribution of these features would be less anomalous in terms of adaptation. On the other hand, a new set of problems would be posed since the positing of a third exploitative zone would present more examples of traits which extend over exploitative boundaries in their distribution.

There are two sorts of circumstances in which ecological approaches do not produce satisfying associations between exploitation and society: first, when features of the exploitative pattern and social features are not congruent in their distributions, and second, when congruency exists but cannot convincingly be demonstrated to be other than spurious. This study has revealed several clear examples of the latter situation and perhaps one or two of the former.

It is perhaps not surprising that associations between exploitative pattern and society have not been found to be comprehensive in this study. To expect such a correlation would imply a more complete espousal of the doctrines of environmental and economic determinism than would be supported by the majority of anthropologists, whatever their theoretical calling.

It should be noted, as well, that even though associations or correlations are noted between society and exploitation or habitat, these do not answer questions regarding origin of social traits. Kroeber (1948: 286) spoke of the integration of internal adaptation in culture and society when he asserted "as regards origin, plan or pattern is secondary." This view can be extended to a consideration of integration with or adaptation of society or culture to external circumstances as well, for whether or not existing features appear to serve an adaptive function in this sense, we are still left with problems of historical events and historical processes if we wish to explain the appearance of a trait in the particular society or culture where it exists.

Historical Factors

Most of the concern with historical factors in social anthropology has focused on diffusion. Murdock (1949: 196) and Eggan (1955*a*: 494) have emphasized that social features seem relatively impervious to diffusion when contrasted to cultural features. Driver and Massey (1957), on the other hand, while acknowledging the role of functional and evolutionary factors in their survey of North American Indian societies, found that in addition to affecting cultural features "changes in social structure are also brought about by contacts of peoples" and "the dynamics of social structure fall within the range of other aspects of culture" (1957: 438).

In order to assess more fully the role of history in social anthropology, the focus should shift from an exclusive concern with diffusion. There appear, in fact, to be several factors involved: *common heritage*, *migration*, *diffusion*, *innovation*, and *drift*. It might be a useful exercise to speculate on the role of these factors or processes in trying to account for the distribution of some of the social features that have been discussed above. The preceding sections of the paper suggest the following table.

Table 3—Summary of distributions for Copper, Netsilik, and Iglulik Eskimo

I. Features shared throughout the three regions:
  - A. Spouse exchange
  - B. Adoption
  - C. Child betrothal
  - D. Kinship composition of maximal groups

II. Features shared by Copper and Netsilik only:
  - A. Seal-sharing partnerships
  - B. Kin endogamy or agamy
  - C. Affinal including uncle-aunt terms

III. Features shared by Netsilik and Iglulik only:
  - A. Family structure and residence
  - B. Composition of minimal groups
  - C. Cross-sex cousin terms

IV. Features individually expressed in each region:
  - A. Distribution networks
  - B. Status differentiation or leadership
  - C. Parallel sex cousin terms

Features in category I have been hypothesized as the results of *common heritage* or of broad common adaptation. The scope of comparison would have to be broadened beyond the limits of this paper to understand more clearly the distribution of these features.

Category II includes features which in their distribution are congruent to the exploitative zones. They suggest the possibility of micro-adaptations. I have supported the correlation of agamous marriage practices and associated affinal, including uncle–aunt terminology, with demographic and exploitative factors but have found less compelling the functional arguments that would support seal-sharing partnerships as being adaptively advantageous. At this point, however, it should be noted that the distributional evidence would tend to support an ecological explanation. These partnerships are not known outside the Copper and Netsilik regions. If we assume then that they have been *innovations* in that exploitative zone, the arguments linking the severe environment with this specialized trait seem to have support.

Since the occurrence of kin agamy, endogamy, or exogamy has not been well documented for surrounding Eskimo areas, it is not possible at this time to determine whether the agamy of the Netsilik and Copper Eskimo areas represents an indigenous modification of a general exogamous pattern, as I have suggested above; or whether it is, itself, a more typical pattern than the Iglulik situation of strict exogamy.

Category III indicates features shared across the boundary dividing the exploitative zones. There would appear to be several explanations in these cases. One might be that the extended family and concomitant social features were generalized Eastern Arctic phenomena (it seems they extended to the Baffinland Eskimo (Boas 1888: 546)) that were adopted through *diffusion* by the Netsilik. Another might be that the nuclear family organization of the Copper Eskimo was an autochthonous trait. The cross-sex cousin terms have been linked to marriage practices in the Iglulik and Copper Eskimo areas, which, in turn, were linked through demography to the level of exploitation. Another explanation would seem to be required for the aberrant Netsilik cross-sex cousin terms. The hypothesis I suggest is that terminology has lagged behind a readaptation of marriage practices. It is conceivable that the Thule ancestors to Netsilik practised kin exogamy and, because of the change to a more marginal subsistence in the ethnographic era, the agamous practices were adopted because of necessity, but the terminology lagged behind. The idea of lag in terminology is acceptable to me because of my conviction, based on studies carried out in a period of drastic social and cultural change, that kinship terminology is an extremely conservative part of Eskimo social organization. The differences in family structure, local group structure, and residence might also reflect a divergent *drift* not relatable to ecological factors.

Category IV would suggest *drift* even more strongly than those features of category III. I shall discuss the applicability of this concept later in the paper.

Here I might suggest that the apparent distinctness of the Copper Eskimo in the number of social features that have been covered in this paper, along with apparently distinctive cultural features, might indicate a later *migration* of ancestral Thule people into the area than that represented by Netsilik and Iglulik area Thule. On the other hand it may also represent one or more of the other processes.

These tentative hypotheses must be followed by more detailed work if progress is to be made in considering the relative weight of adaptive and historical forces and their interaction in producing both uniformity and diversity in social features in the area. Briefly, we might consider the conditions that exist for the operation of these processes and also indicate the direction of possible future research in the three regions considered here, as well as in the Eskimo area as a whole.

Problems of *common heritage* of social traits can be approached by broadening the ethnographic base to other Eskimo areas, but unfortunately our knowledge of social features in the Eskimo area is only beginning to present a coherent picture. Certainly with the relatively recent spread of Thule people and the demonstrable cultural and linguistic uniformity, a substantial body of shared social traits can be expected to exist.

Problems of *migration* of peoples, which could bear some relation to the distribution of social traits, would have to be approached through archaeology, historical linguistics, and, perhaps, physical anthropology, but evidence for social features and their movement along with their bearers would have to be highly inferential and probably would be limited mainly to settlement pattern and, to some extent, household organization.

Evidence for the *diffusion* of material culture traits can be expanded through archaeology and perhaps be linked with the spread of certain social features. The process of diffusion or rather microdiffusion will require careful study. Since intermarriage is the chief means of accomplishing diffusion of social traits, the study of individual cases of marriage between members of the major groupings treated here could contribute to our understanding of how socialization on the parent–child level as well as interaction between adults brings about adoption of social traits across the normal boundaries of the regions inhabited by major groups such as the Copper, Netsilik, and Iglulik Eskimo. In this regard, genealogical work indicates that before 1900 and even later, the Copper, Netsilik, and Iglulik Eskimo areas each virtually comprised a deme within which 95 per cent or more of the marriages took place. This would tend to discount the effects of recent diffusion, a chief factor which affects the distribution of social features in the three societies as they were at contact.

The very conditions of isolation existing in the Central Arctic which discourage diffusion in that area set the stage for *drift*. The concept of *drift* has interesting possibilities for ecological and historical studies. "Cultural drift" has been a subject of concern for a number of anthropologists, but Berreman's (1960) application of the concept to an examination of cultural areas is perhaps closest to my own use of the term in this paper. Berreman (1960: 787) sees drift as "divergent or differential cultural change" which operates under conditions of isolation. *Drift* works on variations produced by innovations which arise from "contact with alien ways of life, by the stresses resulting from new or changing physical and socio-cultural environments, and by chronic discontinuities in or between social and cultural integration" (Berreman 1960: 788).

Eggan (1963) and McFeat (1969) emphasize the role of *drift* in establishing new equilibriums with the environment on the one hand, and in the internal integration of culture and society on the other. These authors focus mainly on variation introduced through culture contact or through changing environmental conditions. For purposes of this study the components of *drift* that concern adaptation to the environment must be separated from those that arise within a society or culture and those that are introduced from outside. In this paper the latter factors have been subsumed under *diffusion*, though the process whereby diffused elements are absorbed into a society or culture has also been designated *drift* (Eggan 1963: 354).

It is chiefly the variations which appear to arise spontaneously within the society that are my concern at this point. One of the critical attributes of *drift* is directionality or non-randomness. Berreman (1960: 792) emphasizes that genetic drift differs from cultural drift[9] in that chance variation is a defining characteristic of genetic drift, while in cultural drift selective advantage plays an important role in determining direction. However, in tracing the process of *drift* back to its beginnings one would have difficulty in finding the basis for selection. Herskovits (1948: 581–2) notes that variants (such as those upon which *drift* acts) do not all have the same dynamic significance. "They tend to be random variations in the sense that they represent all kinds of departures from all sorts of norms. They are dynamically significant only when they begin to accumulate, and thus give direction to cultural change."

Whether these apparently random, internally derived variations arise as Berreman (1960: 788) suggests "from discontinuities in or between social and cultural integration" or from some other source, the social variations which occur in an ecologically uniform area and which do not appear to be caused by external contacts suggest that the process of *drift* may operate along alternate routes in moving toward new integrations.

If social structure is conceived as an abstraction from behaviour varying among small groups within a society, observation of the small groups (Roberts 1951; McFeat 1969) themselves might provide the key approach to solving some of the problems which arise regarding the source of the variation, *innovation*, the problems of role of alternatives in the beginning stages of *drift*, and the factors which determine the direction of *drift*.

Conclusions

Cultural ecology offers the social anthropologist opportunity to break out of circular expositions of society such as are followed in many conventional social structural studies. Ecological approaches offer the possibility of relating to an external system. Correlations between society and environment cannot be convincing unless comparisons are available. A close-ranged comparative study is in many of its aspects actually a study in distributions. Correlations are suggested or invalidated through recourse to examinations of geographical distributions of ecological, cultural, and social features, though certain logical requirements must be met to give weight to these correlations.

In this study I have presented material on three Central Eskimo societies and explored the relationships between the society and the environment as expressed in the exploitative pattern. A number of features of society were found to be related to facts of exploitation, but the relationship was far from complete. Both features of settlement pattern and community pattern were found to relate internally to other social features but not always convincingly to external adaptation.

The distributional aspects of the approach used here seem to lead naturally to a consideration of historical processes, for attempts to correlate society and environmental features eventually pose questions of origins. Investigation of historical factors and processes do not always yield reliable results since varying degrees of researchability are evident in historical problems. At the same time exploration of historical factors will serve as a control for ecological studies. It will also supplement them to elaborate more fully the relationships between societies and the physical, cultural, and social universes to which they relate.

NOTES

1. Field-work which provided much of the background for this paper was carried out in the Iglulik Eskimo area in 1960–61, in the Copper Eskimo area in 1962–63, and in the Netsilik area in 1965.
2. Changes in the aboriginal hunting cycle began to take place slowly around 1915 among the western Copper Eskimo with the introduction of the rifle (Jenness 1922: 123ff.) but not until several years later among the eastern groups. For the Netsilik, rifles were not in general use until about 1920 (Rasmussen 1931: 30; Balicki 1964: 45). In both these regions it was chiefly the pattern of caribou hunting that changed. The animals were hunted over a larger part of the seasonal cycle, for as long as they occurred in quantity.

   In the Iglulik Eskimo region, contacts with whalers had tended to alter the cycle somewhat by the end of the 19th century, especially in the southern part of the area (Mathiassen 1928: 24). The abandonment of caribou hunting from kayaks was one of the first changes.

   In general, however, drastic changes in both the seasonal cycles of economics, and fusion and fission of groupings were not established much before 1925. I have used this year as a rough division between aboriginal and contact-traditional periods, especially with regard to social organization. An important exception was the movement of Netsilik out of their traditional country to the Repulse Bay area about 1900. This resulted in the reduction in size of the major winter groupings in the area indicated as 'Netsilik' on the map. The map thus refers to the period prior to 1900 in this case.
3. Informants from the Netsilik regions of Thom Bay, Rasmussen Basin, and Queen Maud Gulf usually give the hunting of *uuttuq* (*auqtuq* sealing) a minor place in the economy, saying that the practice occupied a period of a week or two when the groups were moving in from far out on the ice. Stefansson (1914: 54) indicates that few of the Copper Eskimo men had much knowledge or skill regarding this practice.
4. Although Webster (*New International Dictionary, Second Edition, 1959*) defines *agamy* as "the absence or nonrecognition of marriage," I have used the term in a different sense in this paper.

   In the absence of any other accepted term I use *agamy* and *agamous* to refer to electiveness in marriage; that is, Netsilik and Copper Eskimos could marry either relatives beyond the primary range or persons to whom kinship connections could not be traced.
5. I am grateful to M. G. Bicchieri for introducing this term. *See* Bicchieri 1969.
6. I have used dialectical variants for several terms. Thus *ihumataq* is Netsilik Eskimo and *isumataq* is Iglulik. *Mauliqtuq* is used in Netsilik and Copper Eskimo areas and *mauliqpuq* in the Iglulik regions, etc.
7. Although Murdock designates neolocal residence as an "Eskimo type" feature, he notes variations in mode of residence in actual Eskimo regions.
8. With regard to these statuses of cousins of the same sex as Ego, I found two systems in the Netsilik area. One classed all the same sex cousins together (Eskimo type), and the other separated them according to the Iroquois pattern. There did not appear to be a sub-regional distribution of these systems. I found the Eskimo type (for cousins of the same sex as Ego only) to be given slightly more frequently by Netsilik informants.
9. Berreman (1960: 792) indicates that his usage of "cultural drift" applies "equally to social structural phenomena and cultural content." This is consistent with the thesis of this paper, and accordingly my usage parallels that of Berreman.

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THE DIFFERENTIAL USE OF IDENTICAL FEATURES OF PHYSICAL HABITAT IN CONNECTION WITH EXPLOITATIVE, SETTLEMENT, AND COMMUNITY PATTERNS: The BaMbuti Case Study

BY

M. G. BICCHIERI

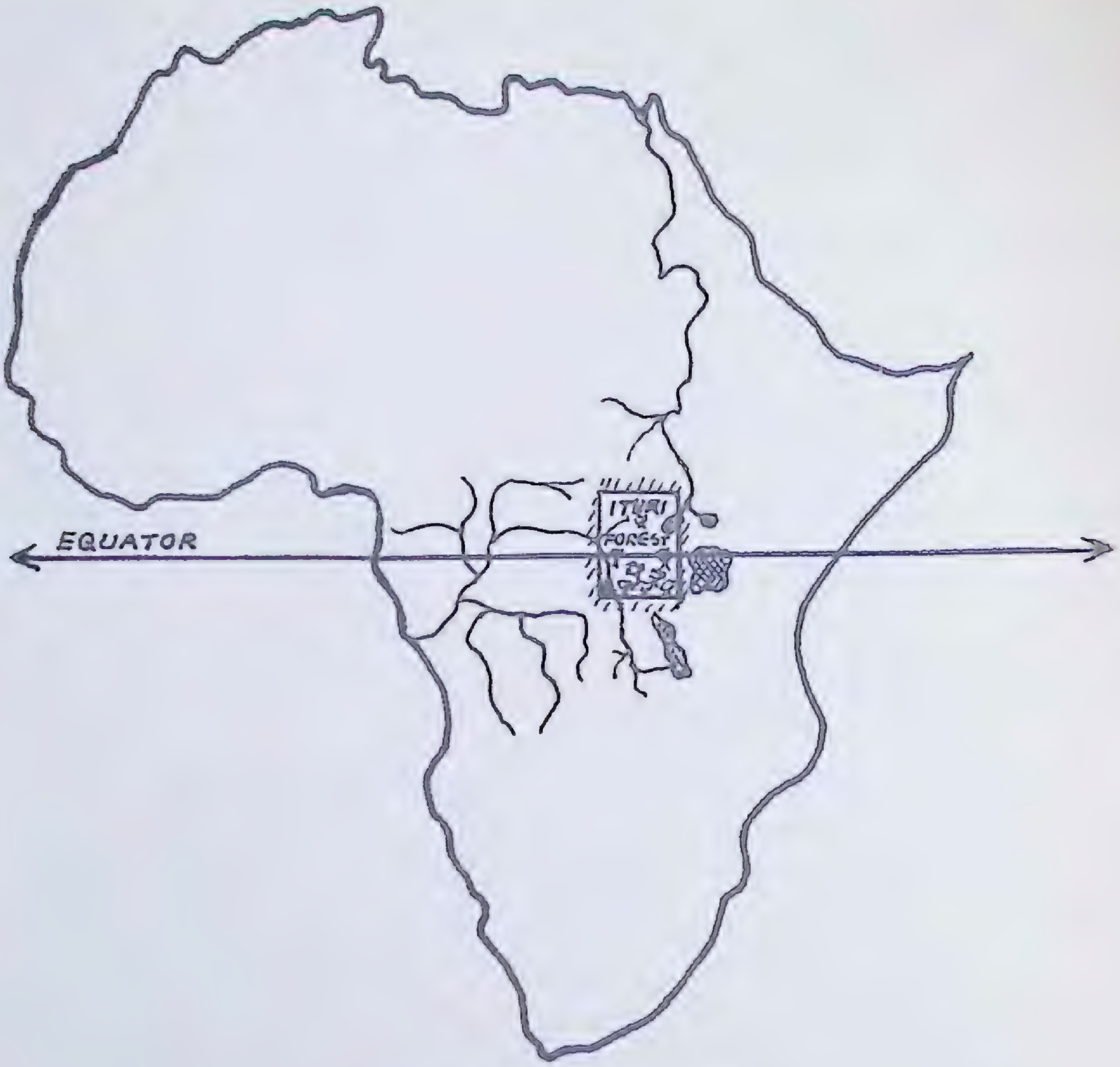
I. Statement of Sources

This analysis, though influenced by a number of theoretical and factual studies, has its roots in certain specific sources: (1) Prof. June Helm's "The Ecological Approach in Anthropology" (1962); (2) Dr. Collin Turnbull's material on the BaMbuti (1964); (3) the Conference on Band Organization sponsored by the National Museum of Canada (1965); (4) personal material from previous research on cultural ecology and food-gathering peoples (Bicchieri 1961, 1963, 1965), as well as ten years of preprofessional contact with the Hadza of Tanganyika. Whenever needed for specific reference, these and other sources will be quoted. The aim in this study is to condense and juxtapose ideas and data from the above material.

II. Delimitation of the Conceptual Frame of Reference

Dr. Helm's survey of "The Ecological Approach in Anthropology" points research toward two phases of inquiry. First it points to the "Redfieldian little community"—also Linton's small group—as the total *natural* unit of investigation (Helm 1962: 638) and, therefore, as the ecological and analytical boundary for initial investigation. Second, the research is directed toward the study of basic relationships between exploitation and socio-organizational principles within the ecumene of delineated communities as the productive emphasis for comparative ecological analysis. The socio-organizational focus, such as the principles of group integration and the patterns of group spatial disposition, constitutes a means of narrowing the investigative frame of reference. On the basis of the results obtained from the restricted socio-organizational focus, propositions can be advanced concerning the existence and the attributes of the socio-economic mechanisms that operate to promote and maintain a specific socio-organizational complex. These types of propositions can serve to transcend typological conclusions (viz., Steward's "patrilineal hunting band") to overcome concepts which are not bound by time and space (viz., F. Eggan on familial and supra-familial organization of the Shoshone; J. Helm on structural bonds in bands' organization; and R. Lee's analysis of Bushmen patterns of exploitation).

Specifically, this study purports to analyse patterns of *exploitation*, *spatial distribution*, and *social integration*. The net-hunting and archer bands of the BaMbuti form the basic ethnological material for this analysis, the differentiation in hunting techniques supplying the comparative dimension that makes this a clear-cut ethnographic exemplification. Utilizing the cultural ecological approach as the heuristic analytical device, suggestions are made (section IV: Interpretation) concerning nature of the interaction pertaining to the above patterns.



Map 1—The BaMbuti location

III. Selected Ethnographic Data

(A) *Ecumene*

Centred on the equatorial belt in the Congo (*see* Map 1), the physical habitat of the BaMbuti is the Ituri primary rainforest, approximately 50,000 square miles of dense, tall forest, which exhibits an annual well-distributed rainfall of some 70–80 inches, a fairly constant temperature of 80°F, and a relative humidity of 95 per cent. The forest not only possesses an abundant supply of water in the form of numerous streams but also produces an abundant and unfailing food supply throughout the year. This abundance includes vegetable foods (edible roots, mushrooms, nuts, berries, and fruits) as well as game (a variety of antelope, birds, monkeys, and insects). As notable as the abundance of essential resources is their temporal and spatial distribution, which is basically uniform both in quantity and variety throughout the yearly cycle, as well as within the forest itself. Honey, a notable exception to seasonal uniformity, is a very important item in the BaMbuti diet, having a focal position in the pattern of social interaction.

Because of the stated concern for ecumene, rather than for aspects of the physical environment alone, the relationship of the BaMbuti to their neighbouring Negro agricultural villagers must be mentioned. The BaMbuti band equates the village with resources and exploits it in accordance with their needs.

This brief description of the BaMbuti ecumene suggests a permissive environment, both in its physical and in its social components, which is consistent with the BaMbuti's own view of their forest world. (More details regarding the permissive–restrictive nature of environments appear in Bicchieri 1965.)

(B) *Patterns of Exploitation*

With the exception of slight localized and temporary variations, the BaMbuti band is equally dependent upon hunting and upon gathering techniques. The basic pattern for securing staples, both vegetable and animal, is greatly influenced by local environmental conditions: *discrete and continuous distribution of resources in a dense forest.* This particular disposition of resources predisposes hunting and gathering techniques, which, in turn, affect social organization. So closely are the techniques and the social patterns related that it is difficult, at times, to see either one as separable from exploitation (Steward's "culture core").

Collecting vegetable foods, though it necessitates a knowledge of particular local conditions, is possible anywhere at anytime and is done mainly by the women, though contribution by men is not uncommon. Among the net-hunters, collecting is a more individual activity, done mainly during a net-drive in which both sexes participate. Among the archers the women who do not participate in the hunt usually go food gathering in small groups.

Hunting, being directed at *mobile resources*, predicates, in spite of its abundance, certain specific *exploiter–resource* relationships. Consideration of the exploiter–resource relationship permits a clearer evaluation of similarities and differences in exploitation techniques and social arrangements between the bands of the net-hunters and of the archers.

Visual contact with prey is difficult because the density of the forest precludes it. This situation does not permit effective utilization of the stalking technique. Consistent with the simple technological level of the BaMbuti, the most efficient method of securing game is to produce a PREY⟶PREDATOR movement. Although it would appear that only the net-hunters, in their net-drive, follow this principle, in reality the archers utilize it as well. Hidden individually or in small groups of two to four members, the archers ambush the prey, which has been brought into range with the help of dogs or game-calls. The prey⟶predator principle is also used by the archers in the annual 'Begbe' beat hunt, which involves a larger number of participants than does the net-drive because they form a 'human net' toward which the game is driven. In the net-drive, a length of net of 30–100 feet, manned by one individual, operates as effectively as perhaps as many as a dozen archers in the 'Begbe.'

The different manner in which the same principle is utilized by the net-hunters and the archers is to be seen mainly as the concomitant of a permissive environment that allows for variations in exploitative devices to coexist in the same ecological niche, the Ituri tropical forest. This stresses the fact that one must look at ecological–cultural covariations not only in a restrictive way but in a permissive one as well. The differential use of the prey⟶predator principle, as an ecologically permissive alternative, has certain significant social correlates. The effectiveness of a net-hunting band depends on a constant numerical membership dictated by the existence of an optimum ratio between the net size and the game killed. If the semicircle formed by the combination of a number of sections of nets is too big or too small, the game will have, respectively, either the time or the room needed to escape the drive.

A net-hunting band would be ineffectual in the game drive if it were to field less than seven lengths of nets because the area covered and the chances of there being any game would be very limited. Conversely, more than thirty lengths of nets would be ineffective because the area covered would be so great that the game would have time to escape the drive. As stated before, nets vary in length from thirty to one hundred feet so that the disparity from seven to thirty lengths does not necessarily mean great disparity in the semicircle obtained. The archers' techniques, except for the annual 'Begbe,' can be efficiently utilized by minimal band sections, usually constituted by as few as one to three families.

Special reference must be made to the honey crop because of its seasonal nature as well as its social implications. Collection of honey, which is almost exclusively the realm of the men, suggests a different exploiter–resource relationship than the one operating in the hunt. In the case of honey, the exploiter must institute an exploiter⟶resource condition. This condition, predicating no loss of productive efficiency through dispersion, produces a most significant socioeconomic situation. During this two-month period, the net-hunting and archer bands show a diametrically opposed socioorganizational response to essentially identical environmental conditions, namely, the abundance of the predilect honey. The net-hunting band fissions into territorially dispersed minimal sections, while the archers, usually dispersed, now unite. The honey season thus allows the net-hunters to relieve the tensions of living together by separating; conversely, the archers are given the opportunity to strengthen ties of friendship and lines of communication. The great ritual significance given by the BaMbuti to honey serves to emphasize their own evaluation of the high cultural significance of this food item.

(C) *Spatial Distribution*

It is now necessary to define the man–land relation to a more specific level than simply BaMbuti–Ituri forest. Concomitant to the abundance and uniformity of the distribution of resources throughout the vast forest and throughout the year, the BaMbuti band is not 'forced' into a specific area. Yet, both net-hunters and archers are very definitely associated to specific territories. It could be suggested, furthermore, that a specific territory, delineated by natural boundaries, 'owns' its band (*see* Bicchieri 1963, 1965). Three conditions, (1) favourable people – land size ratio, (2) natural boundaries, and (3) plentiful and uniformly distributed resources, lead to the presence of discrete territorial areas. A specific band 'belongs' to these *discrete* areas and acts as if it had the resources of the particular territory in stewardship. It is significant that particular BaMbuti bands are named after their particular territory rather than the territory taking its name from the exploiting band. The very fluidity of the band membership continually adjusting to economic conditions, rather than responding to considerations of lineage or leadership, would make meaningless the naming of the territory after members of the band. In the case of individuals, in bands which have become too large or too small in terms of the optimum economic strength of composition, relocation takes place in a *replacive* manner, to realign population to territory, and not vice versa. Notable also is the little concern the people have for trespassers and the fact that resources are considered to belong to the territory where they are acquired rather than to the trespassing acquirer.

Regarding the spatial stability of the specific band operating within a specific territory, it must be noted that the type of nomadism of the BaMbuti is of such a nature that it could be more properly referred to as cyclical migration. Within a naturally bounded territory the game resources tend to migrate, to a limited extent, because of temporary exhaustion of the vegetable resources of a territorial subdivision. The hunting unit generally follows this game migration. Because of the temporary nature of the camp and the easiness with which it can be set up at any point in the 'home' territory, the BaMbuti think of themselves as having a stable pattern of residence, more stable, in fact, than that of the people of the neighbouring village of the Negro agriculturalist who, because of soil exhaustion and forest encroachment, moves his 'stable' residence every few years.

No significant difference exists between the net-hunting and the archer BaMbuti with respect to the band–land relation. The effect of their different techniques of exploitation is reflected at the sub-band level. During most of the year, the net-hunting band is the maximal and minimal operating unit, while the archer band is usually dispersed into minimal band sections made up of even one family but more usually a few families.

(D) *Social Integration*

Having already emphasized territory, we now turn our attention to a brief survey of particular features of social integration and related socioeconomic conditions and mechanisms. The BaMbuti band, whether a net-hunting or archer group, is essentially a composite band with minimal emphasis on lineality (kin principle), particularly male or female lineality. This avoidance of stress on lineality is consistent with the technoterritorial principle which the band adheres to and with the interdependence of the sexes as expressed in economic and social egalitarianism.

Kinship terminology distinguishes sex only at the parental level and is, in effect, only applicable at the level of the nuclear family. Furthermore, kin terms do not differentiate cognates and affines. The significant classificatory statement of relationship concerns generational differentiation. Four age classes — childhood, youth, adulthood, old age — are recognized on the basis of conditions of life and economy, rather than on the basis of degrees of kinship. The existence and persistence of the band is then based on its cognatic nature and on its divisions in horizontal age levels rather than in vertical lineages. The BaMbuti themselves express concern with the perpetuation of the generationally balanced band, rather than with the perpetuation of the lineage.

The band's overall composition is fixed by its territorial–economic relationship, but it is amorphous in its individual or familial membership. The fluidity of the particularized membership is regulated by individual freedom of choice and the mechanism of marriage. While demography and internal organization make marriage within the BaMbuti band possible, they also make exchange-marriage between different and possibly non-contiguous bands essential. Consistent with the principle of economically arranged band composition, residence of newlyweds is defined by the economic consideration of the band's size and composition, rather than by kin or individual preference. The term *replacive-residence* would well express the above situation.

The observable difference in social aggregation between the net-hunters and the archers is manifested in the spatial constitution of their respective bands. As previously stated, the net-hunting band is a closely interacting and cooperative unit composed of a number of familial aggregates during most of the year. The archer band, on the other hand, is broken up into familial aggregates consisting of a few nuclear families distributed throughout the territory to which the band 'belongs.' The differential utilization of the two months of the honey season, as a compensatory mechanism of integration, is a crucial cultural–ecological factor (*see* (B) above).

IV. Interpretation

A conceptual model consistent with the nature of the material and the cultural–ecological method is now advanced to interpret the preceding data on the BaMbuti ecumene, exploitation techniques, and spatial and associational distribution. The premise upon which the analytical lattice is structured focuses on the existence of polarities inherent in the composition of human aggregates (Bicchieri 1965). These polarities are expressed through integrative and dispersive forces which are dealt with in accordance with the community's ecumene and basic technological level. Furthermore, these forces are regulated by means of socioeconomic mechanisms which provide for the achievement of a relative equilibrium between dispersion and integration. The above proposition is best expressed diagrammatically as follows (Tables 1, 2).

Table 1—CONCEPTUAL MODEL FOR ECOLOGICAL ANALYSIS

Basic elements	*Technology*		*Ecumene*	*Group composition*
	material	social organization		(Individual–Group)
Constant condition	Equilibrium conditions between forces . . .		*Forces*	*Manifestations*
			Centripetal ⟶	Fusion
			Centrifugal ⟶	Fission
Adaptive mechanisms	For:	*Economic* ⟷	*Compensating* ⟷	*Socioorganizational*
	Integration	Cooperative production		Integrative devices
	Dispersion	Individualized production		Dispersive devices

Table 2—BaMbuti ecology and the model for ecological analysis

	Technology	*Ecumene*	*Group-composition*
Basic elements	Simple: bow–arrow–digging stick	Permissive Temporal–spatial uniform abundance land and resources. Only seasonality honey. Relation to neighbours non-interdictive toward production	Basic social unit: territorial band, stable size-composition but fluid membership
	NH:* net-drive preponderantly cooperation-dependent at band-unit level		NH: band maximal–minimal basic socio-economic unit Honey-season – Fission
	A:* bow-arrow ambush preponderantly individually oriented at minimal band-segment level		A: minimal band-segment (1–3 families) basic socioeconomic resident unit within band territory Honey season – fuses as band

Constant condition: centripetal and centrifugal forces

	Compensation			
Adaptive mechanisms	Economic		Socio-organizational	
Integration ↗	NH: efficient coop. net-drive technique at band level utilizing interdependence of 4 age-classes	A: efficient and sub-band individualized ambush technique	NH: techniques to prevent excessive friction in closely inter-acting group. Non lineally-bound families–replacive	A: intensive integration dependent on and maximized by honey-season band-fusion
↘ Dispersion	effective utilization of honey-season for familial level exploitation	effective utilization of honey-season for band-level exploitation	familial dispersion utilizing honey-season dispel inter-familial tension. Realignment of family after honey on replacive, age-oriented basis	sub-band unit dispersion achieved through year with lines of band-affiliation changing through marriage-replacement

Note: In Table 2 the corresponding BaMbuti features have been inserted to relate the data to the conceptual model (Table 1).

*NH: net-hunting; A: Archers.

This interpretation has involved, in a reciprocal relationship, the elements of ecumene, technology, exploitation, and group disposition and aggregation. Further detailed analysis will have to be carried out to clarify and juxtapose the various components more systematically. Though rooted in the ethnographical and theoretical material of other investigators, this study may provide a new approach to the analysis of basic–cultural ecological principles. An attempt has also been made to introduce certain new terms (viz., "replacive-residence," prey⟶predator, etc.) which might help to move away from typology-bound analyses and, consequently, to permit the transposition of concepts, free from particularistic labels, from one cultural context into another.

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!KUNG BUSHMAN SUBSISTENCE: AN INPUT–OUTPUT ANALYSIS

BY

RICHARD B. LEE

In this paper the ecological basis of a hunting and gathering economy is examined by means of an input–output analysis of work and consumption. The first goal of this exercise is to outline the subsistence strategy that enables the !Kung Bushmen, with only the simplest of technologies, to live well in the harsh environment of the Kalahari Desert. The second goal is to show that the Bushmen exhibit an elementary form of economic life. And the third goal is to trace, from a primate baseline, the origin and evolution of human energy relationships.

The methodology used is adapted from the transactional models of input–output economics (Leontief 1966) and ecological energetics (Gates 1962; Kleiber 1961). At the outset an essential distinction should be made between these two approaches. Ecologists take as their unit of study a species which has energy relations with other species within an ecosystem. A population is maintained by the energy absorbed in the course of food-getting activities of its members. The focus here is on *interspecific* trophic exchanges (Rappaport 1967: 18–19). Economists, by contrast, focus on the exchange relations within a single species. A productive unit, such as an industry in the American economic system, is maintained by the inputs from other productive units, and in turn its outputs are allocated to other like units or to the 'final demand' sector of the economy (Leontief 1966: 14–20). Viewed ecologically, these transactions can be considered as a highly evolved form of *intraspecific* exchange.

At first glance, the connection between, for example, the relationship of predator and prey in an African savannah and the relationship of industrial units in the American economy seems to be a far-fetched and fruitless analogy. Yet this connection becomes meaningful if we view it in evolutionary terms. We would agree, first of all, that a human population, like all animal populations, has to expend energy in work in order to incorporate energy through consumption. In this respect energetics would apply equally well to the study of man as to the study of other animals.

In one important respect, however, human energy relations are unique among the higher animals. Whereas each vertebrate organism is a self-sufficient subsistence unit, a large percentage of a human individual's energy expenditure goes to feeding others, and a large percentage of an individuals's consumption is food produced by others. Thus for man, the minimal self-sufficient subsistence unit includes at least a social group, such as a family or 'band,' and at most includes economies involving hundreds of millions of people. This central fact of cooperative consumption has been termed 'division of labour' and 'economic interdependence,' and the study of the transactions and allocations so generated forms the foundation of economic science. In man alone, these intraspecific exchanges have become extraordinarily pervasive and complex so much so, in fact, that we take them for granted! Yet it is precisely in this form of trophic exchange that animal adaptation and human adaptation first part company. And it is here that the study of energetics and of economics converge.

In evolutionary terms the origin of what we call an economic system is a relatively recent phenomenon. It appears in the Pleistocene, probably less than two million years ago, when early man began to pool resources and thereby break down individual animal self-sufficiency. Women are usually thought to be the original 'scarce good,' or medium of exchange (White 1949: 316; Lévi-Strauss 1949: 35–86). It is more likely, however, that food was the original medium of exchange and that such exchanges are the foundation of social life.

Many economic transformations have occurred since the basic form of human exchange originated in the Pleistocene. Plant and animal domestication, the development of the market and money, and the harnessing of fossil and nuclear fuels have all contributed to making human energy exchange relations more complex.[1] The evolution of economic organization has reached the point at which an individual's productive activity is usually at the *n*th degree removed from the ultimate source of the food he consumes. He sells his labour (input) to the market and receives his consumption (output) in the form of cash, or some other convertible standard of value (cf., Bohannan and Dalton 1962: 9).

However, economies have evolved at different rates in various parts of the world. In some contemporary societies a much more elementary form of economic life can still be observed. I use 'elementary' in the sense of an economy which exhibits the basic human pattern of exchange, without further elaboration.

In input–output terms, an economy exhibits an elementary form when the relation between the production and consumption of food is immediate in space and time. Such an economy would have the following properties: minimal surplus accumulation; minimal production of capital goods; an absence of agriculture and domestic animals; continuous food-getting activities by all able-bodied personnel throughout the year; and self-sufficiency in foodstuffs and generalized reciprocity within local groups.

Although no contemporary society exhibits all these properties, the !Kung Bushmen of the Dobe area of Botswana are a close approximation. The !Kung have a simple, small-scale, self-contained economy of a type that may have been characteristic of early man. Extreme isolation and a marginal environment have been responsible for the persistence of this form to the present. The Dobe area is bounded in space by waterless desert, and the Bushman population within it is largely self-sufficient in terms of subsistence. The economy lacks trading posts, trade in foodstuffs, wage labour, cash, conversions, and markets — the features which are commonly taken to indicate economic interdependence (Bohannan and Dalton 1962: 1–26). Because the !Kung are hunters and gatherers, without agriculture and domestic animals (except for the dog), and because they do not amass a surplus of foodstuffs, the relation between local food production and consumption is an immediate one. A diagnostic feature of their subsistence economy is: *food is almost always consumed within the boundaries of the local group and within forty-eight hours of its collection.* This immediacy of consumption makes the

!Kung Bushmen an apt case for input–output analysis, since the level of work effort in a given period is a direct reflection of the food requirements of the local group. Such an analysis would be more difficult in a complex economic situation in which the work effort during a given period is dictated by the need for surplus accumulation for ceremonial purposes (Wolf 1966: 7), for the conversion of subsistence goods into prestige (DuBois 1936), or for delayed consumption at a later period (Richards 1939: 35–7).

Sections II through VI of this paper present the descriptive material on Bushman ethnography, demography, and subsistence strategies. These serve as a necessary introduction to the input–output analysis itself, presented in Sections VII and VIII. A concluding section (IX) returns to the problem of defining the properties of an elementary form of economy and attempts to place the discussion in a comparative and evolutionary perspective.

I. Ethnographic Background

The Dobe area lies in the northwestern corner of the Republic of Botswana and in adjacent areas of South West Africa. During my field-work (1963–65), some 336 !Kung Bushmen were resident in the area, along with 340 Bantu pastoralists, mainly of the Herero and Tswana tribes.

!Kung Bushmen are known to have lived in the Dobe area for at least a hundred years. Late Stone Age materials of Wilton Horizon are found at several localities, indicating that some hunting peoples have lived there for many hundreds of years (Malan 1950). There is no evidence that the present-day !Kung are recent refugees from other areas (Lee 1965: 38–68). The introduction of metal tools and weapons can be provisionally dated to the period 1880–90 when iron replaced bone as the primary material for arrowheads and spears.

The first European known to have penetrated the area was Hendrick van Zyl in 1879 (Silberbauer 1965: 115). The Tswana cattle herders appeared soon after, and from the 1890's onwards these pastoralists used the area as a regular summer grazing ground. The first year-round settlements by non-Bushmen were not established until 1925 when two Herero families set up a cattle post at !angwa. Effective administrative presence is even more recent, dating from 1948, the year in which a resident Tswana Headman was appointed by the Paramount Chief of the Batawana Tribal Administration. Apart from brief annual patrols by the British Colonial Government starting in 1934, almost nothing was known of the Dobe area until the 1950's. In 1952, for example, Sillery wrote:

> Not far from the border of South West Africa, near latitude 20° south, is a group of caves which occur in the limestone there. These caves have been visited by few white people. The journey involves a long and arduous trek across sandy country through which no road passes and a competent guide is essential . . . The country in the vicinity of these caves is probably the least known in the whole Protectorate and Bushmen and wild animals have it to themselves. (1952: 198)

The Marshall family of Cambridge, Massachusetts, were the first 'Europeans' to spend more than a few weeks in this area. Their expeditions (1951–59) focused on the adjacent Nyae Nyae !Kung in South West Africa, and their reports are the most complete and detailed record of any Bushman group (L. Marshall 1957, 1959, 1960, 1961, 1962; J. Marshall 1956; Thomas 1959).

In 1960, the South African government initiated a scheme to settle the !Kung Bushmen of the Nyae Nyae area. By 1964 over 700 Bushmen, who had moved into the government station at Tsumkwe, S.W.A., were being instructed in agricultural and stock-raising techniques. As a result of political pressures, the South Africans have not permitted the !Kung Bushmen of the formerly British Bechuanaland Protectorate to participate in this settlement scheme.

The situation in the mid-1960's finds the !Kung on the Botswana side of the border primarily dependent on hunting and gathering for their subsistence. The Bushmen have now obtained some blankets, clothing, and cooking utensils from their Bantu neighbours. There is an inexhaustible supply of metal for arrowheads, which are hammered out from scraps of fencing wire from the cattle enclosures built by the Botswana Veterinary Department. However, the !Kung still lack firearms, livestock, and agriculture. About seven per cent of the Bushwomen have married Bantu; and twenty per cent of the young men are working for Bantu as cattle herders. Serious disputes, usually involving Bush–Bantu labour-relations, are adjudicated by the Tswana Headman. Capital offenders are sent to Maun, the tribal capital, for trial. European presence amounts to a brief patrol once every six to eight weeks. The important point is that the Bushmen continue to hunt and gather because there is no viable alternative available to them.

II. Population Dynamics

In a census made in November 1964, the resident !Kung population stood at 336. Of the 336 residents, 248 were organized into 14 independent camps, ranging in size from 9 to 29 members (*see* Table 1). The estimated size of each camp is a statistical abstraction because individuals and families were con-

Table 1—DOBE AREA CENSUS BY LIVING UNITS: RESIDENT CAMPS

No., place, and name	Males			Females			Totals	Effect.[†]	Dep.	Effect., %
	Y[*]	A	O	Y	A	O				
1. Dobe – n ≠eisi	0	4	1	1	4	1	11	8	3	72.7
2. Dobe – ≠oma//gwe	5	7	2	6	5	1	26	12	14	46.2
3. !angwa – bo	1	6	0	3	5	1	16	11	5	68.8
4. Bate – !xoma	3	7	0	3	5	1	19	12	7	63.2
5. Bate – Liceku	1	5	0	1	4	0	11	9	2	81.8
6. !ubi – Kamburu	3	5	0	1	10	0	19	15	4	79.0
7. !gose – /ise	1	7	0	5	7	2	22	14	8	63.6
8. !gose – /i!ay	0	6	0	1	4	0	11	10	1	90.9
9. !gose – Konguroba	3	4	0	5	7	0	19	11	8	57.9
10. /ai/ai – //aun!a	1	2	1	3	4	1	12	6	6	50.0
11. /ai/ai – //aiha	1	1	1	3	2	1	9	3	6	33.3
12. /ai/ai – ≠oma! xwa	4	9	1	3	10	2	29	19	10	65.5
13. /ai/ai – x!am	2	3	1	5	5	1	17	8	9	47.1
14. /ai/ai – ≠omazho	4	6	2	6	8	1	27	14	13	51.9
Totals in resident camps	29	72	9	46	80	12	248	152	96	61.3

[*] Age Divisions: Y = young, 0–15 years; A = adult, 16–59 years; O = old, 60+ years.

[†] Effectives = adults; dependents = young and old.

stantly shifting from camp to camp. Subsistence at the camps was based on hunting and gathering. The remaining 88 residents were associated with Herero and Tswana cattle posts (Table 2(a)). These people did some work for the Bantu and ate some of their foods, including milk, cheese, and meat. Because part of their diet came from external sources, these Bantu-associated Bushmen groups are not included in the input–output analysis. In addition to the residents, 55 !Kung alternated in and out of the Dobe area (Table 2(b)), and 34 others emigrated permanently from the area (Table 2(c)). Thus the grand total of Bushmen enumerated in 1964 was 425.

Table 2—Dobe area census by living units: other groups

	Males			Females			Totals
	Y	A	O	Y	A	O	
2(a) *Residents living with Bantu* 21 living groups	11	27	2	9	38	1	88
2(b) *Alternators* 6 living groups	13	14	0	12	15	1	55
2(c *Emigrants* 3 living groups	10	7	0	6	11	0	34
Totals 2(a), 2(b), 2(c)	34	48	2	27	64	2	177
Grand total of population	63	120	11	73	144	14	425

In the census, the population is divided by sex and age into three divisions: young, 0–15 years; adult, 16–59 years; and old, 60+ years. Several important demographic features should be noted. Eight per cent of the population in camps (21 of 248) was determined to be over 60 years of age.[2] These data contradict the view that Bushman life expectancy is short. Silberbauer, for example, says of the G/wi of the Central Kalahari that "life expectancy among Reserve Bushmen is difficult to calculate, but I do not believe that many live beyond 45" (1965: 17). Among the Dobe area !Kung, every camp had at least several members over 45 years of age, and ten of the fourteen camps had members over 60 years old; the oldest person was estimated to be 82 ± 3 years. These old people, although non-productive in terms of food, played an important role in the social and ritual activities of the camps.

Since persons under 15 and over 60 years of age do not contribute significantly to the food supplies of the camps, it is possible to use the census data to calculate the percentage of effective food producers and the percentage of dependents. The effectives comprised 61.3 per cent (152 of 248) of the total population in camps; in other words, every three effectives supported themselves and two dependents in subsistence. What is surprising is the wide variation in the percentage of effectives from camp to camp. In camp No. 11 (Table 1), for example, three effectives supported themselves and six dependents (33.3% effective) whereas in camp No. 8, ten of eleven members (90.9%) were effective. These variations were more apparent than real, however, since groups were constantly shifting in composition and the net effect was the production of work groups in which the ratio of effectives to dependents approached the mean.

In addition, these data show an unusual sex ratio favouring females. In the total population, the sex ratio is 84 males per 100 females. For each age group the male : female ratio is: young, 86:100; adult, 83:100; old, 80:100. These data indicate a higher mortality rate for males in all age groups; although it is possible that the sex ratio at birth is anomalous and produces an initial excess of female over male live births. Another possible explanation is the practice of male infanticide. However, the overall incidence of infanticide (as well as invalidicide and senilicide) is so low that it is unlikely that this practice alone would account for the weighted sex ratio in the immature age group.

III. Seasonal Subsistence Patterns

The northern Kalahari Desert is characterized by a hot summer with a five-month rainy season from November to March, a cool dry winter from April to August, and a hot dry spring in September and October. During the spring and summer the diurnal temperature range is from a low of 60°F to a high of 100°F, with shade temperatures of as high as 108°F recorded. In the winter the diurnal range is from a low of 30°F to a high of 78°F. The annual rainfall varies from six to ten inches. The loose sandy soils support a surprising abundance of vegetation, in spite of the fact that the porosity of the sand is such that rainfall is rapidly absorbed and surface run-off is minimal. Permanent waterholes exist only where the underlying limestone strata have been exposed.

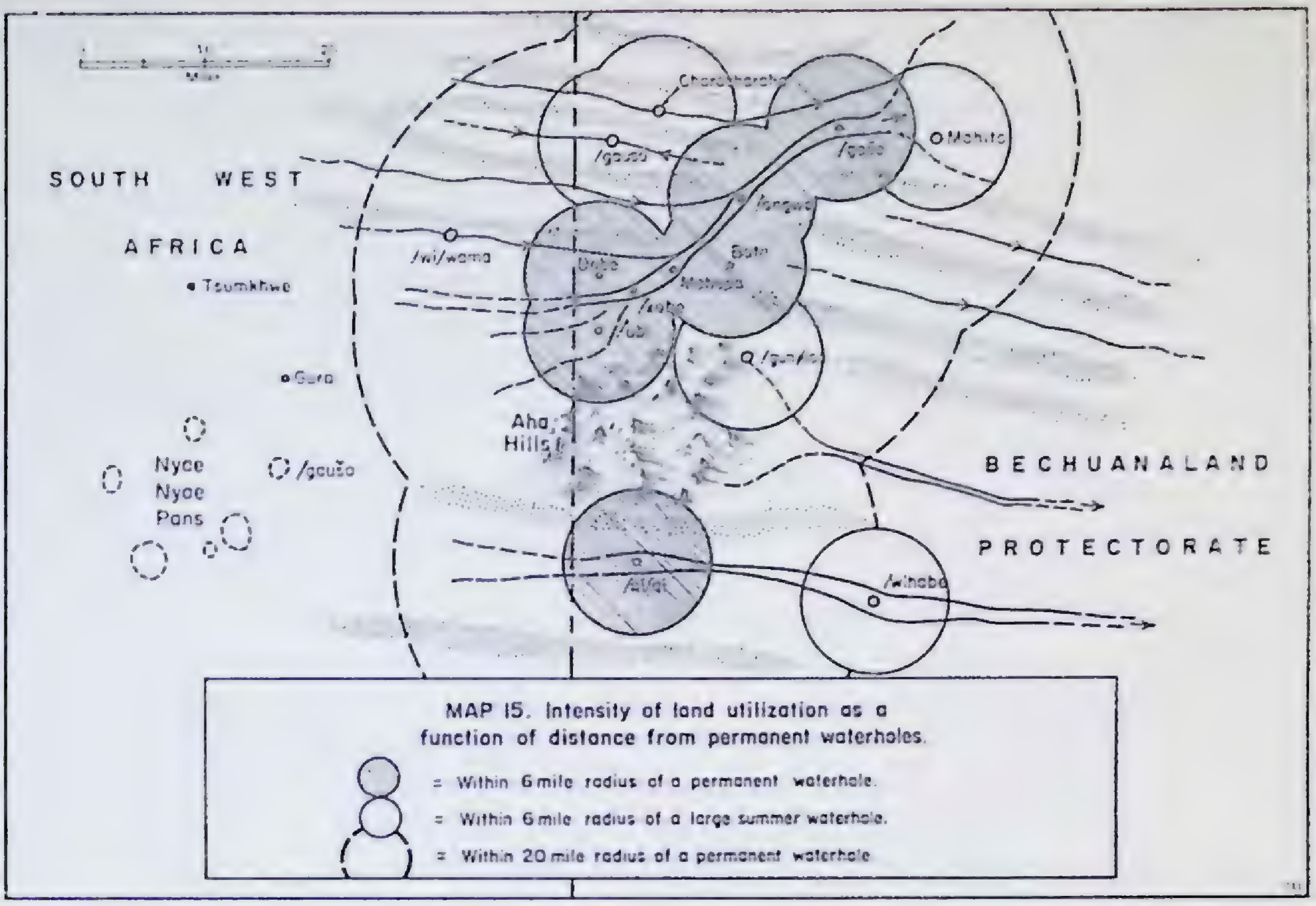
Because of these soil factors, the distribution of water sources, in space and through time, is by far the most important ecological determinant of Bushman subsistence. The availability of plant foods is of secondary importance, and the numbers and distribution of game animals are only of minor importance. Since the Bushman camps of necessity depend on water sources, they can exploit only those vegetable foods that lie within a reasonable walking distance of water. Food sources that lie beyond a reasonable walking distance are rarely exploited.

The life of the camps centred around the eight permanent waterholes are shown as solid dots on Figure 1. During the seven dry months of the year, April through October, these wells were the only sources of standing water, and all the Bushmen camps were located within a mile of a well. During the summer rains (November–March) seasonal pools of water developed elsewhere, and virtually all the Bushmen went out to live at them.

The necessity for drinking water strictly defined the areas that people could exploit for food. On Figure 1 three divisions are plotted:

(1) The shaded circles cover all the areas that lie within a day's walk, i.e., a six-mile radius of a permanent waterhole. This area of about 600 square miles supports all the 248 Bushmen in camps for half the year. Thus the effective population density for the dry season is 42 per 100 square miles.

(2) The unshaded circles enclose an additional 400 square miles that lie within a six-mile radius of a large, seasonal summer waterhole. These areas support most of the resident population for the other half of the year.


Miles
SOUTH WEST
AFRICA
Tsumkhwe
Gura
Nyae
Nyae
Pans
/gaušo
Charotharoha
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/gose
Mahito
!angwa
/wi/woma
Dobe
Bate
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!xabe
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BECHUANALAND
PROTECTORATE
MAP 15. Intensity of land utilization as a
function of distance from permanent waterholes.
= Within 6 mile radius of a permanent waterhole.
= Within 6 mile radius of a large summer waterhole.
= Within 20 mile radius of a permanent waterhole

FIGURE 1—Intensity of land utilization as a function of distance from permanent waterholes

(3) Finally, the dotted line encloses all the areas that lie within a twenty-mile radius of permanent water. None of the Bushmen were observed to camp or forage *outside* this dotted line, and in fact the additional areas, so defined, were rarely utilized by the Bushmen. Therefore about 1,000 square miles was sufficient to support the entire population plus visitors at an effective density of 25 per 100 square miles, or four square miles per person.

Table 3—THE NUMBERS AND DISTRIBUTION OF THE RESIDENT BUSHMEN AND BANTU BY WATERHOLE

Name of waterhole*	No. of camps	Population of camps	Other Bushmen	Total Bushmen	Bantu
Dobe	2	37	–	37	–
!angwa	1	16	23	39	84
Bate	2	30	12	42	21
!ubi	1	19	–	19	65
!gose	3	52	9	61	18
/ai/ai	5	94	13	107	67
!xabe	–	–	8	8	12
Mahopa	–	–	23	23	73
Totals	14	248	88	336	340

*For locations refer to Fig. 1.

The data in Table 3 show the actual numbers resident at each waterhole during the dry season of 1964. Two of the waterholes (!xabe and Mahopa) did not have an independent camp resident. The other six waterholes supported the 248 Bushmen in camps. The mean standing population per waterhole was 41, and the range was from a low of 16 (at !angwa) to a high of 94 (at /ai/ai). These differences in standing population may reflect differences in the density of foodstuffs. It is probable that /ai/ai with 94 residents had more food closer to home than did !angwa, with only 16 residents. Dobe, with a standing population of 37, was closest to the mean and was the subject of an intensive investigation of the relation of subsistence effort to food consumption.[3]

IV. Patterns of Consumption

The camp serves as a home base for its members. Each morning some people move out to collect plant food and (or) hunt game, and each evening the workers return to the camp and share the collected resources with each other and with the members who stayed behind. Food getting is not a cooperative activity. Collectors go out in two's and three's, and each woman gathers plant foods on her own. Hunters usually work individually or in pairs, and the success of the hunt is dependent largely on an individual's tracking ability and on the enthusiasm of his hunting dogs; there is no evidence of coordinated effort producing more meat than individual effort.

Cooperation is clearly in evidence, however, in the consumption of food. Not only do families pool the day's production, but the entire camp, residents and visitors alike, shares equally the total amount of food available. The evening meal of any one family is made up of portions of food from the supplies of each of the other resident families. Foodstuffs are distributed raw or are prepared by the collector and then distributed. There is a constant flow of nuts, berries, roots, and melons from one family fireplace to another until each resident has received an equitable portion (cf., Marshall 1961). When the following morning a different combination of foragers moves out of camp and returns late in the day, the distribution of foodstuffs is repeated. Except in the case of windfalls, such as the killing of a large ungulate, food is rarely taken beyond the boundaries of a camp. People, however, move frequently from one camp to another. The boundary of the camp, therefore, can be considered to define the boundary of the co-consuming group; and the size of the consumption unit will depend on the number of personnel on hand on a given day.

The food resources of the Dobe area were both varied and abundant. I tabulated over 200 plant and 220 animal species known and named by the Bushmen (Lee 1965: 98–121). Of these, 85 plant species and 54 animal species were classified by the Bushmen as edible. The basic food staple is the *mongongo* (mangetti) nut, *Ricinodendron rautanenii* Schinz; alone, it accounted for one-half to two-thirds of the total vegetable diet by weight. This species was so abundant that millions of the nuts rotted on the ground each year. The energy yield of the nut meat is remarkably high: 600 cal/100 g (*see* Section VIII below).

Of the 54 animals classified as edible, only ten species of mammals were regularly hunted for food. The ten species, listed in order of their importance in the diet, are: wart hog, kudu, diuker, steenbok, gemsbok, wildebeeste, spring hare, porcupine, ant bear, and common hare.

V. Foraging Strategy

The Bushmen were observed to be highly selective in their food habits. They stated strong likes and dislikes in foods, and all of the 85 edible species of vegetable food the Bushmen clearly ranked on criteria of desirability: tastiness, nutritional value, abundance, and ease of collection. As a rule, people tended to eat only the most palatable and abundant foods available and to bypass the less desirable foods. Since the other major factor in subsistence was the distance between food and water, it is possible to summarize the basic principle of Bushman foraging strategy in a single statement: *At a given moment, the members of a camp prefer to collect and eat the most desirable foods that are the least distance from standing water.*

Given this principle, the optimum situation occurs when standing water and mongongo nuts are close together, and the worst situation occurs when water and nuts are far apart. The dynamics of the subsistence situation are made clear when we realize that the availability of food is a *stochastic* process: the food that can be eaten in one week is a function of the food that has already been eaten in previous weeks.

The Bushmen typically occupy a camp for a period of weeks or months and eat their way out of it. For instance, at a camp in the nut forests (which form narrow belts along the crests of fixed dunes, *see* Figure 1), the members will exhaust the nuts within a one-mile radius during the first week of occupation, within a two-mile radius the second week, and within a three-mile radius the third week. As time goes on, the members of the group must travel farther and farther to reach the nuts, and the round-trip distance in miles is a measurement of the 'cost' of obtaining this desirable food.

In Figure 2, the cost of obtaining mongongo nuts is plotted against distance. The cost curve for nuts rises slowly as the round-trip distance increases from two to twelve miles, climbs sharply from twelve to sixteen miles, then levels off for longer distances. The reason for the inflection in the cost curve is the difference between one-day trips and overnight trips. A round trip of up to twelve miles can be accomplished in a single day, but for trips to more distant points an overnight hike, which involves the packing of drinking water and the carrying of heavy loads over long distances, must be organized.

The alternative tactic to the longer trips is to stay at the home base and to exploit foods of lesser desirability in terms of taste, ease of collection, and abundance. At a given dry-season camp one sees both tactics in evidence. The older, less mobile members of the camp stay close to home and collect the less desirable foods, while the younger, more active members make the longer trips to the nut forests. As the water–nut distance increases, more and more attention is given to the less desirable foods.

During the rainy season, a different and less costly strategy is employed. Temporary pools of water form at a number of localities so that when nuts are exhausted within a few miles of one pool, the entire group can move camp to another pool where water and nuts are still abundant. Thus the 'cost' of obtaining nuts during the rainy season never exceeds the level of a six-mile round trip. Towards the end of the rainy season, a temporary pool may dry

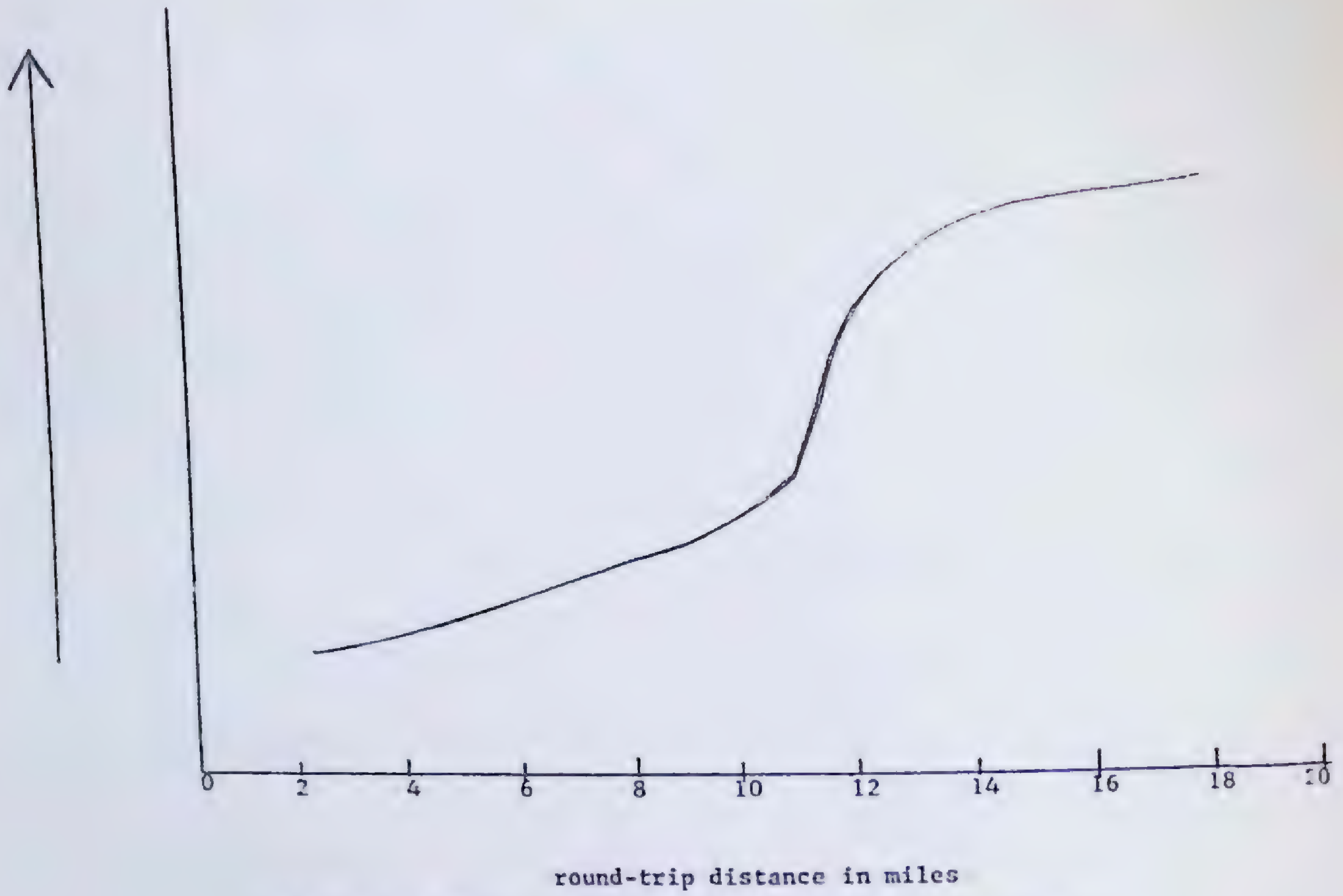

FIGURE 2—The cost curve for obtaining mongongo nuts

up before the nuts in its immediate vicinity become exhausted. In this case the residents move camp to one of the large summer waterholes which usually persist until early fall (April or May). When these latter pools dry up, the entire population moves back to the areas around the eight permanent waterholes to spend the winter and spring dry season.

It is evident that the crucial factor in the annual subsistence cycle is the distance between food and water. Basically, the Dobe area !Kung face three different sets of conditions through the year.

Condition I. Food Abundant and Many Water Points

During the rainy season (November–April) all the people live at the temporary pools in the midst of the nut forests. The water–food distance is short, and the subsistence effort is minimal. This is also the season of plant growth when seasonal foods such as berries and leafy greens are available.

Condition II. Food Abundant but only Eight Water Points

In the early half of the dry season (May–July), all the groups are based at the permanent waterholes. They eat out an increasing radius of desirable foods. As the water–food distance increases, the subsistance effort increases.

Condition III. Food Scarce and only Eight Water Points

By the end of the dry season (August–October) the water–food distance approaches an annual maximum. People must either walk long distances to reach the nuts, or be content to eat the less and less desirable foods, such as bitter melons, roots, Acacia gum, and the heart of the vegetable ivory palm. The diet is most eclectic at this time of year.

With the onset of the first rains in late October or November, a new cycle of plant growth is initiated, and seasonal pools again form in the hinterland. The subsistence effort decreases to the level of *Condition I.*

VI. Input–Output: The Subsistence Effort

As stated earlier, the purpose of Bushman work is to get food, and the amount of work expended is therefore a measure of the effort required to feed the group. In addition, the food gathered is equitably distributed among all members of the camp and rarely moves beyond camp boundaries. With these points in mind, we can apply the framework of input–output analysis to the Bushman data. The work input, or subsistence effort, is a compilation of all the days of work carried out by members of a group within a specified period of time. The subsistence effort could be stated in terms of the number of work days per week per hunter or gatherer. This however is a crude measure since it does not define the size of the consumption unit. In addition, one should know the number of dependents who are being supported by the work. The consumption unit, therefore, is defined by adding the total number of effectives to the total number of dependents resident at a camp during a specified period.

I have found the following formula useful for measuring S, the index of subsistence effort:

$$S = \frac{W}{C} \times 100$$

where W = the number of man-days of work and where C = the number of man-days of consumption.

Example 1—Consider a hypothetical population of 10 people subsisting for a 30-day period. Since everyone eats every day, the value of C (man-days of consumption) is:

$$C = 10 \times 30 = 300$$

How many man-days of work will be necessary to provide 300 man-days of consumption? If every person worked every day of the 30-day period, then:

$$\begin{aligned} W &= 10 \times 30 \qquad \text{and} \qquad & S &= \frac{W}{C} \\ &= 300 & &= \frac{300}{300} \\ & & &= 1.00 \end{aligned}$$

Since every person works every day in order to eat every day, the value of S is unity.

Example 2—If everyone worked on alternate days, then:

$$S = \frac{10 \times 15}{300} = .5$$

Example 3—If half the people worked every day and half the people were dependents, then:

$$S = \frac{5 \times 30}{300} = .5$$

Example 1, in which everyone works every day, is not merely a hypothetical case. In fact, $S = 1.00$ approximates the situation among the non-human primates (and most other vertebrates) in which every animal (save nursing infants) forages for its own food on every day of the year. For monkeys and apes (DeVore 1965) the value of S approaches unity, and the actual value of S is simply a function of the percentage of nursing infants in the population.

A baboon troop, for example (Hall and DeVore 1965: 70), leaves the sleeping area each morning and spends the day moving as a group through its range. Although the spatial cohesion of the group is maintained, each troop member acts as a self-sufficient subsistence unit, collecting and eating its own foods as it moves. There is no *exchange* of food among individuals; this is truly a 'hand-to-mouth' existence. The foregoing is not meant to imply that subsistence is precarious for baboons and other primates. On the contrary, the individual animal may spend only a few hours a day obtaining food, and this activity is interspersed with periods of social grooming, sexual and dominance behaviour, and sleeping. The point is that the work rhythm is such that every individual must do some subsistence work on *every single day of his adult life*.

The work rhythm of human groups is radically different. All human societies allocate some days to work and others to leisure, and in all human societies some people work harder than others. However, the condition $S = 1.00$ can be regarded as the baseline from which man evolved. The sharing of food is part of a cluster of basic human institutions which also include the division of subsistence labour, the home base, the primary carrying device (for transporting foods to the home base for distribution), and the prolonged support of non-productive young and old people. These developments represent a quantum step in human affairs, for their presence means that not *all* of the people have to work *all* of the time.

The immediate implication of division of labour was that the value of S, the index of subsistence effort, must have dropped radically during the early phases of hominid evolution. It is at this point that human economics parts company with animal energetics. The long-term implications of division of labour are manifold. Exchange opens up the possibility of more and more complex forms of surplus accumulation, either for the purpose of distribution to a wider social group or for the purpose of consumption by the producers themselves at a later date. Therefore one of the important dimensions along which economic evolution can be traced is the increasing separation between the production of food and its allocation to consumers.

Formally, the !Kung Bushman economy corresponds to an early stage in this trend since the relation between production and consumption of food is immediate in space and time. Food produced by the local group is consumed within its boundaries, usually within 48 hours of production. The major concern therefore, is to use the formula $S = \frac{W}{C}$ for the analysis of !Kung subsistence.

In any self-sufficient human group the magnitude of S, the subsistence effort, is a function of the ease or difficulty of feeding the group. One would assume that hunter–gatherers such as the !Kung Bushmen, with a simple technology, living in a marginal desert environment, would have a difficult time getting food; therefore the index of subsistence effort should be relatively high.

For instance, in a group of ten people, if the six adults had to work $5\frac{1}{2}$ days per week to support themselves and four dependents, then the value of S would be about .5. A $5\frac{1}{2}$-day work week is not excessive by Western industrial standards. On the other hand, if the work week were only three days long, then the value of S would fall to .26.

Table 4—DOBE WORK DIARY: A RECORD OF THE ACTIVITIES AT THE DOBE CAMP FOR THE 28-DAY PERIOD, 6 JULY – 2 AUGUST, 1964

Week	Date	1 Adults	2 Children	3 Man-days of consumption[*]	4 Man-days of work[†]	5 Meat output, lb
I	July 6	18	9	27	9	–
	7	14	9	23	6	92
	8	15	9	24	2	–
	9	15	9	24	3	12
	10	16	9	25	7	–
	11	18	11	29	3	–
	12	18	9	27	7	–
II[‡]	13	20	11	31	5	–
	14	16	9	25	0	–
	15	16	9	25	1	–
	16	14	9	23	0	–
	17	19	12	31	11	80
	18	17	9	26	3	–
	19	23	14	37	2	–
III	20	26	14	40	9	110
	21	24	11	35	3	24
	22	19	13	32	3	–
	23	18	11	29	4	27
	24	23	13	36	10	16
	25	22	10	32	6	–
	26	24	12	36	7	–
IV	27	22	13	35	12	7
	28	27	13	40	12	80
	29	26	13	39	9	10
	30	24	11	35	16	12
	31	22	10	32	4	20
	Aug. 1	24	11	35	8	–
	2	22	11	33	16	–

[*] Column 3 documents the variation in camp population from day to day and from week to week.

[†] Definition of a work day = a day in which one person collected food for the camp or a day in which one man went hunting.

[‡] Week II (July 13–19) shows an unusually *low* work output. The investigator contributed food on July 12 and 17, resulting in a decreased subsistence effort for the seven-day period. Week II therefore has not been included in final calculation of the *S* ratio (*see* Table 5).

KEY: Column 1: daily count of adults in the camp
Column 2: daily count of children in the camp
Column 3: number of man-days of consumption ((3) = (1) + (2))
Column 4: number of man-days of work (a daily count of the residents who engaged in subsistence activities)
Column 5: daily meat output in pounds of edible meat

The calculation of the actual level of subsistence effort is, of course, an empirical question. Table 4 tabulates the four-week work diary for the Dobe camp during the period July 6 to August 2, 1964. This period was chosen because it was neither the easiest nor the most difficult time of year for subsistence, and it covered the period of transition from better to worse conditions.

Column 2 shows the number of adults at the camp on each day, and column 3 the number of children. Column 4 tabulates the number of man-days of consumption (and incidentally documents the daily variations in group size). Column 5 indicates the number of people who went out for food each day. Column 6 lists the meat output, in pounds of edible raw portion, for each day.

Table 5—SUMMARY OF DOBE WORK DIARY

Week	1 Mean group size	2 Adult-days	3 Child-days	4 Total man-days of consumption	5 Man-days of work	6 Meat lb	7 Work week	8 Index of Subsistence Effort
I (July 6–12)	25.6 (23–29)	114	65	179	37	104	2.3	.21
II (July 13–19)	28.3 (23–37)	125	73	198	22	80	1.2	.11
III (July 20–26)	34.3 (29–40)	156	84	240	42	177	1.9	.18
IV (July 27–Aug. 2)	35.6 (32–40)	167	82	249	77	129	3.2	.31
Total	30.9	562	304	866	178	490	2.2	.21
Adjusted totals*	31.8	437	231	668	156	410	2.5	.23

* *See* note † Table 4.

KEY: Column 1: $\text{mean group size} = \dfrac{\text{total man-days of consumption}}{7}$

Column 7: work week = the number of work days per adult per week

Column 8: $\text{Index of Subsistence Effort} = \dfrac{\text{man-days of work} \times 100}{\text{man-days of consumption}}$

(e.g., in Week I, the value of $S = .21$, i.e., 21 days of work per 100 days of consumption or 1 work day produces food for 5 consumption days)

Table 5 is a summary, by week, of the work diary. Mean group size (column 1) varies from 25.6 to 35.6; the actual count of personnel on hand was rarely the same for two consecutive days. The work week (column 7) varies from 1.2 to 3.2 work-days per adult. In other words, each productive individual supports herself or himself and dependents, and still has 3½ to 5½ days available for other activities. The Index of Subsistence Effort (column 8) varies from .11 to .31. For instance, during Week I (July 6–12) 37 man-days of work were expended to provide 179 man-days of consumption. The value $S = .21$ indicates 21 days of work per 100 man-days of consumption; or each day's work provided food for the worker and four other people. During Week IV (July 27–August 2) 77 man-days of work provided 249 man-days of consumption for an S value of .31 (31 work days per 100 consumption days). The work input during Week IV is 50 per cent higher than in Week I. This rise reflects an increased difficulty in reaching food, although in terms of actual time devoted to the food quest, the average rises from two days per week to three per week per individual producer.

In calculating the overall average value of S for this period, I have omitted Week II for the reason noted (Table 4,‡) therefore, the Index of Subsistence Effort for this camp of !Kung Bushmen is .23. Since the non-productive members comprised 35 per cent of the population, another way of expressing the Index is to say that 65 per cent of the people worked 36 per cent of the time, and 35 per cent of the people did no work at all.[4]

Two of the ecological conditions noted above are represented in the work diary. The first week is condition II, in which food is abundant but only eight water points are available. People are making the daily round trips to the nut forests, giving an S value of .21. By the fourth week condition III has appeared; it is no longer possible to reach the nuts in one day since all the nuts have been eaten within a radius of over seven miles. The round-trip distance to the nearest nuts is over fourteen miles, and the cost curve of nuts has turned sharply upwards (*see* Figure 2). The higher value of S (.31) reflects the marked increase in overnight trips to reach the nut forests.

VII. Input–Output: Caloric Levels

Having considered the level of work effort required to feed the group, it is necessary to determine the quantity of energy yielded by this work effort. Since the actual time devoted to subsistence is modest, the question arises whether this low work effort produces a substandard diet.

The major constituents of the diet by weight during this period were:

	Per cent
1. Mongongo nuts	33
2. Meat	37
3. Other vegetable foods	30
Total	100

During field-work no direct caloric observations were made. It was difficult to measure a single individual's daily food intake since this was eaten over a period of several hours in the late afternoon and evening and was made up of small portions from the supplies of different families. However, since foodstuffs are shared equitably throughout the camp, it was possible to measure gross per capita intake by estimating the total weight of food brought in and dividing it by the number of people on hand. A net per capita intake figure was calculated by deducting values for waste (inedible portions, bones, nutshells, etc.) and allowing for loss through cooking. An account of the methods and results follows:

1. The staple mongongo nut is particularly suitable for this kind of analysis; it is easy to count and weigh, and the percentage of edible constituents is precisely known. The nut consists of a hard outer shell and a soft inner shell (both inedible) and a core of edible nut meat. The whole nut weighs 5.0 g, and the nut meat comprises 14 per cent of the total weight, or 0.7 g (Anon. 1917; author's field observations).

There are approximately 200 whole nuts per kilogram (91 nuts per lb). Each kilogram of whole nuts yields 140 g of nut meats (64 g per lb). I weighed the total back load of nuts brought in by a sample of women each day. A woman's daily collection of whole nuts weighed between 10 kg (22 lb) and 15 kg (33 lb), although back loads of as much as 20 kg (44 lb) of whole nuts were recorded. Each back load contained on the average 2,500 whole nuts, as well as smaller quantities of other foodstuffs.

Since the edible portion of whole nuts is 14 per cent, each 12.5 kg back load of nuts contained 1,750 g of edible nut meats.

Records were also maintained for the number of nuts cracked and eaten by individuals and families on a single day. Women roast a quantity of nuts in the coals of the fire for a few minutes before cracking them. The nuts are equally palatable when raw, but the brief roasting serves to drive off some of the moisture and makes the hard outer shell easier to crack. The nut is then cracked open, using a fist-sized limestone cobble as a hammerstone and a larger flat limestone block as an anvil. The extreme hardness of the shell accounts for the remarkable storage properties of the mongongo. Nuts are still perfectly edible after having lain on the ground for a year.

The cracking and shelling rate averages five or six nuts per minute and varies little from one woman to another. In one hour a woman cracks and shells 300 to 360 nuts, or one-eighth of a back load, and an hour's cracking yields 210 to 252 g of edible nut meats. On the basis of observations of cracking rates and time devoted to cracking, and on the basis of total weights of whole nuts brought into the camp, Bushmen were observed to eat about 300 nuts per person per day; this yielded 210 g of nut meats. Thus, one back load of whole nuts would feed a family of four for two days, with a little left over for the third day.

The constituents of the nut meat have been determined (Wehmer 1931, vol. 2: 678),[5] and the nutritional yield can be calculated (Oser 1965: 1336).[6] The yield is 600 (±1%) cal/100 g of edible portion, and the protein yield is 27 g/100 g. The caloric value of mongongo compares favourably with that of domesticated species of nuts, such as almonds (600 cal/100 g), brazil nuts (653), cashews (563), and peanuts (583). In proteins, however, it exceeds the levels for these other nuts (27 per cent for mongongo vs. an average of 19 per cent for other species).

2. Complete records were kept of game animals killed, and of the quantities of meat brought into the Dobe camp during the 28-day period of the work diary. Eighteen animals, totalling 206 kg (454 lb) of edible meat, were killed and consumed by members of the camp.[7] In addition, 16 kg (36 lb) of meat were brought into Dobe by visitors from other camps, for a total of 222 kg (490 lb) of meat. Dividing this figure by the 866 man-days of consumption (*see* above) gives a daily allotment of 256 g (9.1 oz) of uncooked meat per person. Even allowing for a ten per cent shrinkage in cooking, the caloric yield of this allotment is estimated at 690 calories (based on a rate of 300 cal/100 g,

cooked). The protein content is estimated to be 15 per cent by weight, or 34.5 g per cooked portion.

3. The remaining vegetable portion of the diet consisted of small quantities of 20 species of roots, melons, gums, bulbs, and dried fruits. No caloric observations were made for these foods, and their total caloric yield is estimated at 100 cal/100 g. Protein yield is negligible and is estimated at one per cent.

Table 6—CALORIC AND PROTEIN LEVELS IN THE !KUNG BUSHMAN DIET, JULY–AUGUST, 1964

Class of food	Contribution to diet, %	Per capita consumption		Calories per person per day
		Weight, g	Protein, g	
Meat....................	37	230	34.5	690
Mongongo nuts..........	33	210	56.7	1,260
Other vegetable foods.....	30	190	1.9	190
Totals.................	100	630	93.1	2,140

In Table 6 the three main food sources (meat, mongongo nuts, other vegetable foods) are brought together in order to show the contribution each makes to the Bushman diet and to derive an estimate of daily per capita intake of calories and proteins. The results show a daily allotment of 2,140 calories and 93.1 g proteins per person. Because of the high protein values for mongongo, the protein intake is unusually high, even by American standards. It is also unexpected that a hunting people should get such a high proportion of their proteins from vegetable rather than from meat sources.

Does a per capita intake of 2,140 calories meet the energy requirements of the group? The Bushmen are small in stature and weight. The average height and weight for adult males are 157 cm and 46 kg, and for adult females 147 cm and 41 kg (Bronte-Stewart *et al.* 1960). Basal metabolic requirements for individuals of such heights and weights are calculated at 1,400 calories per day for males and 1,100 calories per day for females (Taylor and Pye 1966: 45–8). Given an activity régime that varies from light-moderate to severe exercise (including an hour of nut-cracking per day and two 10-mile hikes per week), the caloric requirements can be estimated at 2,250 calories for males and 1,750 calories for females, per day of an average work week. These figures apply to adults thirty years of age and would necessarily be less for middle-aged and elderly persons. For children I have taken a median age of eight years for all individuals under 15 years and estimated the daily requirements to be 2,000 calories (Taylor and Pye 1966: 463).

To calculate the daily caloric requirement for the study group as a whole, it is necessary to take an average, weighted according to the percentage of each age–sex class in the population. Since the population consists of 30 per cent adult males, 35 per cent adult females, and 35 per cent children under 15 years (*see* Tables 1 and 2), the mean daily energy requirement for a group of 31 persons is 61,300 calories, and for each group member, about 1,975 calories.

The per capita yield of foodstuffs during the study period was estimated to be 2,140 calories (Table 4), and therefore it is clear that food output exceeds energy requirements by about 165 calories per person per day.

These data indicate that the even modest level of subsistence effort noted in Section VII is more than enough to provide an adequate diet for the !Kung Bushmen. The conclusion can be drawn that the Bushmen do not lead a substandard existence on the edge of starvation, as has been commonly supposed.[8]

Part of this caloric surplus is absorbed by the food portions allocated to the maintenance of hunting dogs. The dog population of Dobe varied from five to eight animals. The dogs eat what is left over when people have eaten their fill; and it is worth noting that the physical condition of the animals seemed to show more seasonal variation than the condition of the humans. It may be possible that, in input–output terms, Bushman dogs consume most of the marginal variation in abundance of foodstuffs brought into the camp.[9]

The remainder of the caloric surplus may go into physiological accumulation of fat by the Bushmen during the good season, an accumulation which is then metabolized during the worst season of the year (September–October). Future research should include the weights and skin-fold measurements of individuals taken each month through the annual cycle. During the lean season of the year, the availability of the staple mongongo nut reaches an annual low, and the people have to walk farther and work harder in order to maintain an adequate diet. In other words, a higher energy input yields a relatively lower caloric output.

The significance of the differential activities of young and old people can now be appreciated. The more able, more mobile members of the group have higher energy requirements, and they have the means to meet these by making the long hikes to the mongongo nut forests. The old people, with more modest energy requirements, remain close to home and gather a more eclectic diet of low-yield roots, bulbs, and edible gums. The group as a whole distributes the collective resources in such a way that the caloric needs of each age–sex class are met. In input–output terms this is a way of restating the classic dictum: *from each according to his means and to each according to his needs.* In principle the Bushman camp is a communistic society. In practice, sharing is never complete, but conflicting parties have the option of rearranging themselves spatially so that, when sharing breaks down, new groups can be constituted to ensure parity of production and consumption.

The input–output approach to subsistence has shown that !Kung Bushmen in the Dobe area can derive a more than adequate living from only a modest expenditure of their time and effort. The analysis may help to correct the impression that their life is a constant struggle, maintained in the face of adversity, and ending in early death. As Sahlins has pointed out in a recent discussion (in press), our view of the hunter has been conditioned by the traditional wisdom of the economics of scarcity. We have tended to equate poverty with the absence of material wealth. Sahlins suggests the alternative interpretation that hunters may be simply in business for their health, and that this modest end can be achieved even with the rudimentary technical means at their disposal. The result is that hunters may actually enjoy more leisure time per capita than any other level of society (*see also* Service 1966: 13).

This intriguing possibility is confirmed by the quantitative measurements of !Kung Bushman subsistence. Although food-getting is the primary productive activity of the Bushmen, most of their time (four to five days per week) is spent in other pursuits, such as resting in camp or visiting other camps.

Since the northern Kalahari Desert is by any account a marginal habitat for human occupation, it is likely that hunters in the past would have had an even more substantial subsistence base. Today the remaining hunters are confined to the least attractive environments of the world, but in Pleistocene times they would have had their pick of the richest areas, in terms of game, plant foods, and water supply.[10]

VIII. Elementary Forms and the Logic of Generalized Reciprocity

One of the most striking cross-cultural regularities yet discovered is the almost universal practice of voluntary food sharing among small-scale hunter–gatherers.[11] Sahlins has labelled this practice 'generalized reciprocity' and defines it as the giving of food, or other goods, without a definite expectation of return (1965: 147). It is the kind of transaction that obtains in our own society between members of the nuclear family, and it falls at the solidary (sociable) extreme of Sahlins' continuum, ranging from generalized, through balanced, to negative reciprocity, the latter being the unsociable extreme (1965: 147–9). Service has observed that the widespread sharing of foodstuffs within hunter–gatherer local groups is a function of the fact that the band is a community of close kinsmen. Generalized reciprocity is therefore simply an extension of the familistic ethic to include the entire local group (1966: 24).

Viewed as a system of allocations, generalized reciprocity may be a necessary sociological outcome of the elementary form of economic life defined in Section I of this paper. The clue lies in the implications of this practice for the organization of subsistence. The obverse of sharing is, of course, hoarding, or withholding. The latter is reported to be a cardinal sin among hunter–gatherers (Service 1966: 18; Sahlins 1965: 200–1, 215–18). Yet hoarding is but a morally negative paraphrase of the respectable economic term 'surplus accumulation.' The act of setting aside a portion of one's production for consumption or distribution at a future date is the essence of bourgeois economics ('savings'), but among the hunters it is regarded as stinginess or hard-heartedness. Since everyone in a hunter camp must be fed from the food supply on hand and since no one can be refused, the constancy of demand tends to keep food inventories at a minimum. It also tends to maintain 'wealth' differences between individuals at an exceedingly low level. Constant turnover and low inventories are simply different facets of the earlier definition of an elementary form as an economy in which food production and consumption are immediate. In such an economy, the withholding of food by even one party would be incompatible with the model of generalized reciprocity. Only if all parties are equally wealthy—or to be more accurate—equally poor, can the economic equilibrium be maintained.

It is quite possible that one individual in such an economy, against the expectations of his fellows, would decide to husband his resources and allocate his production to savings rather than to sharing. The short-term result of such a move would probably be ostracism for the individual; but if enough of his fellows were to follow his example, then the social fabric would be preserved and a new economic equilibrium would be established at a higher level of surplus accumulation. Wealth disparities would now become possible, and an avenue for the conversion of subsistence goods into prestige would open up. Such a society has embarked on the road to 'economic development.'

In human energy relations, no individual is self-sufficient. Human existence is, at base, made possible by the work effort of individuals, but social life is founded upon the principle of cooperative consumption of resources. It is fortunate for anthropologists that in some contemporary societies the rudimentary forms of exchange may still be observed. In other societies higher orders of complexity can also be observed, enabling the analyst to trace the evolution of economic systems.

Starting from the primate baseline of a 'hand-to-mouth' existence, one can discern several secular trends in human social evolution. One such trend leads to an increasing separation in space and time between the production of food and its final allocation in consumption. Another is in the direction of conversion of an increasing proportion of subsistence output into the production of durable goods. And a third trend is toward a greater and greater disparity in the distribution of wealth among individuals.

On all of these dimensions, the !Kung Bushmen exhibit an elementary form. Although the ideology of exchange is complex, the formal aspects of exchange are simple. Using input–output analysis may contribute, on a quantitative level, to our understanding of the distinction between nature and culture and the evolution of economics.

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NOTES

1. The implications of these developments have been documented by Marx (1867), Childe (1951), Polanyi (1944, 1957), and White (1949, 1959).
2. The age estimates are based on a relative age ranking of the population from youngest to oldest; an event calendar was used to establish birth dates. The accuracy of estimation is ±3 years.
3. The ecological and demographic determinants of group structure will be discussed in another paper.
4. In calculating the Index, I have only taken into account the work actually devoted to getting food. The time spent on manufacturing the tool kit has not been included, nor has the time spent on processing food been included. However, in calculating the caloric requirements (Section VIII), I have included a value for the energy expended in such activities.
5. Fats 59.4%, Protein 27.0%, Crude Fiber 5.9%, Water 4.7%, Ash 3.02%.
6. Modified Atwater formulas used by the F.A.O. were employed, based on the following values: 8.37 cal/g of fat, 3.4 cal/g of protein.

7. Edible: waste ratios for various mammals were calculated by R. H. S. Smithers.

8. The possibility that the Bushmen were enjoying an exceptionally good year can be discounted. The observations were made during the second year of a severe drought, which seriously dislocated the pastoral and farming economics of the Bantu but apparently did not seriously affect the foraging economy of the Bushmen. If drought conditions demanded a three-day work week from the Bushmen, then one would have to postulate an even lower work input during years of average or higher rainfall.

9. Pigs may play a similar role in the subsistence economy of Melanesians (Vayda *et al.* 1961; Rappaport 1967). Unlike the Melanesians who eat their pigs, the Bushmen have never been known to eat their dogs.

10. The reconstruction of the prehistoric habitats of hunter–gatherers has been discussed in detail elsewhere (Lee 1963, in press).

11. Some of the evidence has been brought together by Sahlins in his excellent review on reciprocity 1965: 186–91, 200–1, 215–18). The practice of generalized reciprocity within local groups is found among: Mbuti Pygmies, Andaman Islanders, Australian Aborigines, Eskimos, Semang, and Great Basin Shoshone. For references consult Sahlins (op. cit.).

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SOME ASPECTS OF KHOIKHOI (HOTTENTOT) SETTLEMENT PATTERNS IN HISTORICOECOLOGICAL PERSPECTIVE

BY

W. PETER CARSTENS

The somewhat barbarous adjective 'historicoecological' has been used in the title of this paper because I wish to pledge allegiance to the theory which states that relationships in space, social or otherwise, do not really differ from those in time, and both are equally important. Moreover, the concept, diachronic, becomes tautological if we define the synchronic approach to the study of society, culture, and environment in terms of systems of interrelated parts. Parts related spatially will also be related temporarily — in *one* system.

In this brief analysis of Khoikhoi settlement patterns, I shall accept the contention that the Bushmen hunter-gatherers inhabited the whole of southern Africa from an early unknown date (Schapera 1930: 26–7) and preceded both the Khoikhoi herders and the Southern Bantu. The corollary to this proposition is reflected in the subtitle of Stow (1905):

> A history of the intrusion of the Hottentots and Bantu into the hunting grounds of the Bushmen, the Aborigines of the country.

The Khoikhoin, therefore, like the Bantu-speaking peoples and the Europeans, entered the territory as colonists. Of their social organization prior to the mid-seventeenth century, virtually nothing is known. But of their adaptation to their organic and superorganic environments before the impact of European culture and technology we have some useful early accounts (Axelson 1954: 1–13; Jopp 1960). The history of the Khoikhoi (1652–1966) is well documented (Van Riebeeck 1953; Cape of Good Hope 1835; Stow 1905; Schapera 1930; Marais 1939). At the risk of over-simplification, Khoikhoi social organization and settlement patterns will be examined through time using three historical periods characterized as follows:

(i) The Khoikhoi of the seventeenth century
(ii) The territorial revolution: spatial and social (1700–1847)
(iii) Modes of adaptation to reservation systems and missionary regimes (1847–)

The Khoikhoi of the Seventeenth Century

The early European colonists soon recognized and reported the existence of various kinds of 'Hottentots' and classified them according to their respective habitats, economies, and, to a certain extent, their intertribal relations (Van Riebeeck 1953; Kolben 1738). Although population estimates are largely absent for these tribes, it may be inferred from the early records (Kolben 1738; Cape of Good Hope 1835) that both the size and density of settlements were greatest

(a) When the natural environment permitted year-round grazing for their zebu cattle and fat-tailed sheep (Epstein 1937);
(b) When fish were plentiful and conditions for trapping and angling were favourable;[1]

(c) When a strong military organization was necessary to protect kith, kin, cattle, and country, with the proviso that the military organization was established for territorial protection only and not for expansion or plunder;

(d) When *veldkos* and game were plentiful.

The Hessequa tribe in the vicinity of Cape Town was probably the largest and most powerful of all the Khoikhoi and possessed all the above characteristics.

At the other end of the spectrum were the so-called *Strandloopers* (Beach-rangers) who roamed the shores of Table Bay and 10 to 20 miles to the north. Essentially food gatherers, especially shellfish, they numbered less than one hundred and were a subgroup of one of the Cape Khoikhoi tribes who migrated east after striking the southern tip of Africa (Van Riebeeck 1953; MacCrone 1937: 18, 45). The point that I wish to make is that these food gatherers were originally of the herding tradition but changed their mode of subsistence and social organization for *both* geographical and social reasons. On the one hand they broke away from their tribe probably because of political rivalry. On the other hand the only land available was the sea shore north of Table Bay (Kolben 1738: 61–82; Goodwin 1952).

The remaining Khoikhoi tribes displayed variations in organization, which can be explained by their adaptation to numerous phenomena, the two most important being the much emphasized physical habitat and the often neglected military factor. For example, writing of the Attaqua, Peter Kolben says:

> The Attaquas are possess'd of but a very indifferent Soil, and but ill provided with Water. For which Reason they live in little Troops at a considerable distance from one another, in Parts the most commodious. 'Tis owing to this too, that they are not overstock'd with Cattle, having, for the most Part, only just so much as, with the Game they catch, is necessary to sustain 'em. They are, however, as brave, contented and lively as if they enjoy'd the most flourishing Country. They live in great Tranquility, and are seldom at War with their Neighbours. When they are in Danger of an Enemy, their Method of assembling their Forces is this: They hasten, like the Switz, to the Tops of their highest Mountains; where they make Fires that shall cast up a great Smoak by Day, and a clear Flame by Night.[2] Upon this Signal, every one, who is able to serve, hastens with his best Arms to one constant Place of Rendezvous; and there is immediately assembled a very numerous Army. (1738: 71–72)

This brings me to the question of population distribution and its variations among the Khoikhoi tribes. Schapera (1930) tends to present a generalized account of encampments and dwellings based on both the Cape tribes and the Namaqua. Moreover his distinction between the patterns of "the old days" and "nowadays" implies change effected by acculturation only, rather than the more significant ecological processes. Drawing on various sources he states:

> In the old days the main tribal encampment was in the form of a great circle. The whole was enclosed with a great fence of thorn, in which were two gateways, one to the north, the other to the south. Within this fence and round the circumference were the huts of the people, each hut facing inwards to the centre. (1930: 228)

Schapera goes on to state that the open space created by this pattern served as a fold for the livestock at night.

It seems doubtful to me that each Khoikhoi tribe established this pattern for all its members. In the first place early writers (Jopp 1960: 180–1; Kolben 1738: 217–26) make it quite clear that these kraals varied in size from 20 huts (about 100+ people) to 100 huts (about 500+ people). This suggests that these groups were clans and not autonomous tribes or political communities (Carstens 1966: 20–1). Secondly, there is evidence that this circular residential pattern was a function of habitat and expediency (Schapera 1956: 12–13). Thus we find that it was essentially the southern Cape tribes who adopted this settlement arrangement. The explanation seems two-fold:

(i) The high population density (both cattle and men) in the area initiated by the ethnological cul-de-sac and made possible by the Mediterranean climate, increased the importance of having clearly defined boundaries (the territory factor) and adequate protection (the military factor).

(ii) The problem of keeping livestock together in areas where both water and food are plentiful is extremely difficult. In the harsh semidesert climate of the northern Cape and South West Africa, people and their livestock are restricted to those localities where water is available. If they did, in fact, form circular encampments in the north, these were essentially military centres, or laagers, for general protection from the sorties of the Bushmen into whose territory they had intruded. It could in fact be argued that the circular encampment was developed in an embryonic form during the southerly migration of the Khoikhoi.

It is clear then that there was considerable variation among the seventeenth century Khoikhoi regarding settlement patterns. The explanation is both symbiotic and social, as I have tried to show. If geographical environment alone could explain all these variations, there would be greater morphological value in attempting to distinguish precisely between the 'Cape Hottentots,' 'Eastern Hottentots,' and the Namaqua. It is true that there is a relatively high mathematical correlation between climate and settlement pattern for the tribes constituting each of these categories, but the exceptions are too significant, as earlier examples have illustrated, for this assertion to be made.[3]

The Territorial Revolution (1700–1847)

I shall deal with this period briefly, and only because I must establish a link with the later period characterized *inter alia* by the reserve system. During the next 150 years Khoikhoi culture underwent a complete metamorphosis. Some tribes or parts thereof became incorporated in the settled Colonial population at the Cape, where together with the Europeans and slave population they formed the urban and rural Cape Coloureds.[4] Others retained their nomadic pastoral element unmodified and formed an integrated (or synthetic) culture with the European frontiersmen who had now also become pastoralists. These became known as the *Basters* (Fischer 1913; Carstens 1966). Other tribes or remnants of tribes moved north and settled largely in South West Africa, while the Eastern tribes were engulfed and assimilated by the vanguard of the Southern Bantu (Schapera 1930: 43–7).

During this period of transition the territorial factor was being taken care of first by the Dutch, then by the British Colonial Governments, and finally by the missionaries. Thus when the colonial boundary was extended to the Orange River in 1847, the framework of the territory to which the remnants of the Khoikhoi (and amongst these we must include the *Basters*) had to adapt was firmly established. The missionaries had already established ritual and educational centres close to the best springs and waterholes (Marais 1939; Carstens 1966).

Modes of Adaptation to Reserve Systems and Missionary Régimes (1847–)

At the present time there are in South and South West Africa reserve communities all situated on semidesert or desert land. Some are regarded as Nama Khoikhoin, others as Basters. Of particular relevance here are the differences between their internal sociocultural systems, especially differences in the structure of local groups. This part of my discussion will be confined to five South African reserves.[5] All are situated in the district of Little Namaqualand and are almost contiguous. They have similar physical environments although less rain falls in the northern than the southern reserves. All have had similar histories: they grew up around mission stations which fostered integration between various groups of Khoikhoi and Basters into communities of the peasant variety; from time to time they have admitted outsiders to *burgherskap*, while others have emigrated permanently to the towns. They learned plough agriculture from the missionaries and now sow grain, generally to their economic disadvantage even when adequate rain has fallen. But basically they still regard themselves as herders, for although cattle are rare, goats, sheep, and donkeys now dominate the herding aspect of their mixed economy. Informants in all these communities tend to portray their kinship systems in much the same terms, as though they all conformed inflexibly to one set type. Thus they report that there is strict separation between the sexes, that descent is patrilineal, that authority is patripotestal, that residence is virilocal after a period of uxorilocality, and that the basic residential unit (apart from the nuclear family) is the patrilineal extended family.

A closer look into the systems revealed that there are wide and significant variations from the informants' accounts, the origin of which is not entirely clear. For example, in the most 'traditional' and isolated of these five communities several characteristics were discovered

(i) Only 9.3 per cent of the families were of the patrilineal extended variety. Moreover, 16.2 per cent were matrilateral extended families, and the majority of people lived in families containing affinal kin. Also permanent uxorilocality was found to be common.

(ii) Although there is a measure of sexual separation in this community, it is not a rigid one. Husbands and wives interact closely in domestic affairs; men and women gossip freely together; girls often assist in the herding; and in general the status of women is higher than in any of the other communities. This pattern, moreover, is exemplified in the layout of the dwelling units, where the cooking hut is constructed only a few feet away from the living hut. Thus women can commune more easily with men, even while they are carrying out domestic chores. It was found here that women are constantly asserting their position and presence by interrupting male conversation with their comments. Even if this pattern of behaviour emerged in the other communities, the greater distance (50 feet or more) between the two huts constituting a dwelling unit would restrict this communication considerably.

It is difficult to explain this tendency, especially as the community concerned has no agriculture[6] and relies largely on herding and a little on food gathering for its livelihood. In looking for possible solutions to the problem I have found it rewarding to direct my attention to the other communities for an answer, particularly because patrilineal bonds are strongest in that community where the cultivation of grain is economically most profitable. The overall evidence suggests that the practice of agriculture introduced by the missionaries in the 1820's has tended to reinforce patrilineal ties through land ownership, land being acquired from agnates. And complementary to this is the cooperation in ploughing, reaping, and threshing, which is carried out by male patrikin.

These kinship units in their various forms are integrated within their respective communities. Unlike the Cape Khoikhoi of the seventheenth century, the spatial distribution of the population is related to the reserve system, missionary endeavour both sacred and secular, as well as to physical habitat. Thus the main village in each reserve is built round the mission station which established and still controls most aspects of the education system; the largest hamlets, moreover, are those which have schools, used also as ritual centres; the minority of the population lives away from schools and churches.

On looking broadly at the settlement patterns of these communities it is evident that, with the establishment of mission stations and later reserves to protect the rights of the religious community and to maintain external control over the political community, a substitute for the military or garrison factor of the seventeenth century has been instituted.[7] These mission reserves, however, provide instead social refuge areas which guarantee protection, provided the rules of the church and the local and central governments are adhered to.

None of these communities is self-sufficient, and all have to depend on outside employment to augment their local needs. More than 60 per cent of the income for these communities is derived from external sources, migratory labour being the most rewarding. The institution of migratory labour, moreover, has become an adaptive factor to encapsulated reservation life, for it enables larger numbers of people to participate in village life. However, without migratory labour the cultural coalitions effected by the church and the schools could not be maintained. These generalizations do not, of course, provide a moral justification for the institution of migratory labour.

It is perhaps relevant to record also that migratory labour is greatest where the churches are most active, where missionaries advocate its benefits, and when those who temporarily escape reservation life are pursued by church officers to the mines. Two of the objectives of this benevolent activity are to extract church dues from the workers and to ensure that earnings are sent back to the home community.

It should, therefore, not be difficult to see how a social institution like the church, which involves itself in the community life in general, also has the direct effect of facilitating adaptation to the natural habitat at the level to which it has been locally exploited. Moreover, the size of the villages and the density of their populations do not appear to be related to such factors as water supply alone. For instance, in the community closest to the Khoikhoi tradition the dwelling huts are widely dispersed; even extended families tend not to cluster together. If water, a precious commodity, was an overriding factor in the settlement pattern, one would expect clusters of huts round the waterholes and springs. But the very converse is true, and some families have to walk up to two miles to draw water, although there is plenty of suitable building space. In this community, religious activities of all kinds are infrequent. It has no resident missionary, only occasional ritual celebrations, and a poor understaffed school.

To draw conclusions from these few observations and data concerning the Khoikhoi in their various stages is not easy — especially difficult is it to relate them to the main theme of this conference. It would appear, however, for the Khoikhoi groups that have been considered, given a constant environment and exploitation thereof, that with the absence of both the garrison factor (protection) and the religious factor (ritual and educational), local groups tend to be small and discrete, even though this may be to their economic disadvantage.

Rogers' paper in this volume raised many relevant factors which may be connected with the dispersion of groups under these environmental conditions. The people I studied live in constatn fear of getting themselves involved in *sorcery* situations, and it is responsible to argue that group fragmentation is an avoidance response to this fear. The question, however, remains: why is this centrifugal trend found among the members of one community and not among others? Perhaps the answer lies in what Mrs. Hoernlé (1923) called the "social value of water."[8] Springs and waterholes are few and basic to survival. All people must use them and maintain a maximum degree of harmony, a condition which the people themselves would find impossible if they lived within 'fighting' distance of each other.

NOTES

1. It is often thought that the Khoikhoin were essentially herders; but most tribes, if not all, did in fact rely greatly on fishing as a source of food.
2. The shrub used would appear to have been the inflammable candle bush, *Sarcocaulon barmanni.*
3. I am grateful to Professor G. P. Murdock for helping me clarify this point of view, although he may not fully agree with it.
4. For a general account of this process see Marais (1939).
5. This material is based largely on my own field-work (*See* Carstens 1966, especially chapters 4, 11, and 12).
6. Hoernlé (1913) reports that in 1913 there was some evidence of land that had been used for agricultural purposes, but she was not very much impressed with its economic value. She implies, moreover, that the land had not been ploughed for many years.
7. It should be noted that the military settlements referred to here were of the Nama Khoikhoi type, and not of the more concentrated Cape Khoikhoi type.
8. Dr. Audrey I. Richards suggested to me in conversation recently that the "biological value of water" would have been a more appropriate phrase.

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ECOSYSTEMS IN PROCESS: A COMPARATIVE STUDY OF CHANGE IN TWO WEST AFRICAN SOCIETIES

BY

ROBERT McC. NETTING

Ecological studies are obviously riding a mild wave of popularity in current cultural anthropology. The titles of papers read at professional meetings, a rash of published articles, several imposing research projects, and this seminar all bear witness to this increased interest. Graduate students, those sensitive barometers of what is currently *in* in the discipline talk about 'niche' and 'territoriality,' 'cultigens,' and 'symbiosis' with all the assurance and pretentiousness that once accompanied discussions of cross-cousin marriage or modal personality. Certainly the faddishness of ecology will lead to the usual amount of mediocre output, but there remain some valid reasons for the serious attention now being given to this approach.

In the first place, we now have on record a solid set of ecologically oriented studies by Julian Steward (1937, 1938, 1955), Barth (1956, 1961), Sahlins (1958, 1962), Geertz (1963), Vayda (1961), and Scudder (1962) that demonstrate how useful this type of analysis can be in organizing and understanding cultural data. Moreover, they have laid to rest both the exaggerations of environmental determinism and the essentially negative reaction of possibilism.

Secondly, in terms of strategy and method, I suspect that the holistic approach of biological ecology which includes both the human organism and cultural behaviour in relating man to his physical environment is intrinsically congenial to anthropological tastes. For a generation eager to demonstrate limited hypotheses with sounder documentation and quantitative evidence, ecological analysis has particular attractions.

Finally, there seems to be a widespread feeling that studies of social structure and function alone are becoming sterile, that social facts may not always be best explained by other social facts, and that culture is essentially a coping mechanism for adapting to the environment (Worsley 1956; Gray 1964: 6–9; Winans 1965; Goldschmidt 1959). A structural–functional analysis may leave one with the 'too-perfect' equilibrium which cannot account for or accommodate the dynamics of change. Max Gluckman (1963: 38) has recently rejected the organic analogy that had been an article of faith among his contemporaries. He noted that social systems are not nearly so integrated as organic systems and that social processes are not so cyclical nor so repetitive as those in organic systems. "Moreover, social systems are open to the influences of changes in environment, and to changes due to other social systems, as organic systems are not. I think therefore much more in terms of series of social processes, operating within an ecological setting and the biopsychical frameword of human life, as well as of the restriction and action of a technology and a culture." (Perhaps this rejection will initiate an era of slightly less *social* anthropology across the Atlantic.) At the very least, it is plain that a study of social systems is incomplete without some understanding of the ecosystems in which they operate.

Today the ecological viewpoint is well represented by a growing body of studies which provide clearly defined sets of functional interrelations. We can now begin to analyse and compare them, to inquire which are primary and secondary variables, and to discern some line of causality or at least a certain priority of factors. As Steward *et al.* (1955: 71) noted in discussing the recurrent relationship between irrigation, population density, and particular forms of political organization, "It is definitely implicit in our approach . . . that we seek causes." The pursuit of causality, whether it is ultimately successful or not, leads to controlled cross-cultural comparisons and historical considerations which are required to make our demonstrations of functional interdependence really convincing. Indeed "the very meaning of functional dependence is that change in one variable results in change in a dependent variable" (Spoehr 1950).

I believe it is useful to advance propositions on the systematic functional relationships of selected factors of environment, productive technology, and social institutions based on synchronic ethnographic data. My own field-work has been on the Kofyar, a group of some 55,000 subsistence farmers living on a 200-square-mile tract of tumbled rocky hills and plain fringes in Northern Nigeria. They are a collection of Chadic-speaking village groups referred to in the scanty literature as Kwolla, Dimmuk, Mirriam, and Koffiyer (Ames 1934). Though traditionally lacking any political unity, the Kofyar peoples share certain cultural characteristics which distinguish them from neighbouring 'hill pagan' groups, the Sura, Angas, and Tal, as well as the lowland Goemai or Ankwe. Elsewhere I have suggested that the rainfall, soil, and terrain of the Jos Plateau, the techniques of intensive agriculture practised by the Kofyar peoples who live there, and certain features of demography, settlement pattern, work-group organization, and land tenure are functionally interdependent (Netting 1965). I have also tried to chart covariation through time of some of these factors, particularly farming methods and household composition, as the Kofyar moved from subsistence cultivation in the hills to cash cropping on vacant plains land (Netting 1965*a*). Briefly stated, the variables which appeared to be associated were as follows:

(1) A physical environment with substantial and dependable supplies of rainfall (40–60 inches per year) resulting from moist southerly winds rising and cooling as they encounter the plateau, pockets of fertile soil on hilltops of decomposing granite and alluvial fans, and a rugged, abrupt escarpment of 1,500 to 2,000 feet affording protection from outside invaders, especially cavalry.

(2) An agricultural system combining terracing, annual manuring, stall-feeding of livestock, and raising densely interplanted grain, vegetable, and tree crops on homestead farms. Permanently intensive cultivation is thus maintained without soil depletion or erosion. Subsidiary shifting cultivation is carried out on bush fallows with crop rotation.

(3) High density of population (300 per square mile) and villages of dispersed homesteads, small households (averaging 5.3 individuals) with strong statistical emphasis (90%) on non-extended families, and individual tenure and inheritance of land.

In the last thirty years, and especially since 1950, there has been a sizeable movement of Kofyar to migrant farming about thirty miles south of the Plateau. The new land, which is level and is easily worked, has a high initial fertility. The Kofyar pioneers used only the methods of shifting agriculture, cutting and burning the trees and moving on when harvests began to diminish. Since individual farms were large and were widely spaced, population density was much lower than in the traditional villages. The pattern of dispersed homesteads was maintained, but households became larger in size with more polygynous marriages and a greater frequency of extended families (Netting 1965*a*). The new land was not acquired outright but remained the property of a local Goemai village, administered by its chief who granted usufruct rights in return for annual tribute. Thus, the change situation illustrates a certain correlation in the alternation of farming techniques, land availability, population density, household organization, and land tenure patterns.

Obviously the case for a real functional relationship of these factors would be improved if their existence and their co-variation could be demonstrated in another context. Recent work on the Igbo of eastern Nigeria by a series of geographers and anthropologists provides evidence of this kind. Igbo subsistence is based on the shifting cultivation of tropical forest lands for yams, cassava, and coco yams along with the gathering of palm products (Forde and Jones 1950). Traditionally, villages belonging to a village group were positioned around a central meeting place from which areas of farmland radiated like the sectors of a circle (Jones 1949; Udo 1965). After blocks of land within each sector were cleared annually by communal labour, the village then assigned usufruct rights to its various members. Fallow lands remained under communal control. Each village was made up of one or more localized patrilineages, but major productive groups appear to have been the extended family households including a man, his wives and children, and the families of his married sons.

Pronounced social changes, often referred to as "disintegration" (Jones 1949; Udo 1965), have been noted in some parts of Igboland. Nucleated settlements have broken down and with them the large extended family compounds. Nuclear or small independent polygynous family households have dispersed evenly over the landscape, farming more or less permanent gardens of vegetables and tree crops with fine-comb techniques and frequent fertilization. Land has passed largely into individual control with rights to alienate it temporarily by lease or pledge, or outright by sale (Field 1945). The areas where these practices prevail are characterized by severe population pressure of 400 to well over 1,000 people per square mile. The maps in Udo (1965) show a central area where heavy rural population correlates with dispersed compounds. This area runs roughly north and south and contrasts with the nucleated villages, compact forest hamlets, and street villages to the east, west, and south. This process of social dispersion and individualization combined with an increasing intensity of horticulture involves the same factors noted among the Kofyar, but the change is in the opposite direction. The two clusters of variables and the direction of the shift are represented in Table 1.

The salient question to be considered here is the nature of functional interaction among these variables and the causal sequence by which they change. We might suspect first that the alterations of the system are due to changes in the physical environment. However such features as rainfall, temperature, soil

Table 1—SYSTEMS OF LINKED VARIABLES

	Population density	Agricultural system	Settlement pattern	Household organization	Land tenure
↑ Kofyar	High	Intensive	Dispersed	Non-extended	Individual
Igbo ↓	Low	Shifting	Nucleated	Extended	Communal

fertility, and topography have remained relatively constant. The Igbo have adopted new methods of cultivation and social organization while remaining in the same place. Though the Kofyar have moved farther onto the plains as migrants, the land is not unlike that at the foot of the plateau escarpment, which they have farmed for generations. The fertility of the new land resembles that of the fully regenerated bush near the traditional villages. The crucial difference is not the quality of natural resources in either case but their abundance, specifically the amount of land available for farming.

Though technological innovation is usually given a prominent place in explanations of social change, there seem to have been no new tools or methods introduced among either the Kofyar or the Igbo. The hoe and the axe, or matchet, remain the major implements. Ploughs, animal power, and new crops are absent, and there has been no appreciable intervention by government agronomists or developers in either situation. The voluntary changes reflect rather a shift in emphasis on existing techniques. The Kofyar merely expanded their use of bush fallowing which had always been an adjunct of intensive homestead cultivation. The Igbo progressively extended the manured kitchen garden which adjoined the compound and bore root crops and vegetables in the shade of oil and raffia palms, oil beans, and African breadfruit trees (Jones 1945, 1949). In such land-hungry areas as Ezza in Abakaliki, compost of grass, household refuse, and animal dung was prepared in circular pits, and night soil was systematically collected (Wallace 1961).

It might be possible to contend also that changes in social organization are largely traceable to contact with Western notions of nuclear family independence and individual land tenure. Though foreign contacts have been important in both cases, especially mission education and the introduction of a cash economy, it is difficult to assign the changes we have been discussing to these factors alone. There is no evidence of direct pressure being exerted to break up the Igbo extended family, and the government has generally opposed dispersed settlement because it interferes with the provision of social services. According to Jones (1949: 321), "Foreign influences have affected Igboland tenure very little." The Kofyar have actually diverged from Western patterns, going from small independent family households to extended families with an increased occurrence of polygyny. Communal tenure is replacing a strongly individualized traditional pattern.

As we consider and discard various possible explanations for these changes, it becomes increasingly clear that the pressure of population on an existing land base is the triggering factor. Land scarcity can originate from demographic growth within a restricted area, as was the case with the Kofyar who were confined to their protecting hills for safety from militarily stronger neighbours.

The area for exploitation may also be limited when possibilities for migration and colonization become nonexistent with the occupation of all arable land and the enforced prohibition on predatory expansion (cf., Jones 1961). With a growing number of cultivators competing for subsistence on a fixed land area, the advantages of intensive crop production are obvious. It is too often overlooked that intensive methods without corresponding improvement in technology imply an increase in the amount of labour expended relative to food units produced. The intensive cultivator works harder for his daily bread than his shifting (or shiftless) brethren (Clark and Haswell 1964: 33, 47). Terrace building and elaborate ridging, maintaining domestic animals for manure, careful hoeing and weeding, transplanting, multi-cropping, water control, and conservation all involve more working hours than slash-and-burn field preparation (Boserup 1965: 43–55). Given the fact that "the best general rule to the behavior of primitive farmers is that they work to get the maximum return for the minimum effort" (Nye and Greenland 1960: 129), the effort will only be intensified when the maximum return, under the old system of agriculture, is no longer enough to maintain a desired standard of living. Conversely, if population pressures are relaxed by the opening of new land, even fully competent intensive cultivators such as the Kofyar will quickly revert to less strenuous farming methods (cf., Boserup 1965: 62).

The effects of increasing population density are reflected directly in the quantitative indices of agricultural production. The most crowded areas of Igboland show food shortages, smaller and higher priced tubers, more reliance on fruit and imported vegetable produce, and a net export of labour (Forde 1946: 77). Morgan's (1953) comparison of three neighbouring communities indicates that as population density increases there is a regular decrease in years of fallow and average size of holding (Table 2). Among the Kofyar, those villages with the heaviest population, the most overworked soil, and the greatest scarcity of bush land were the first to engage in migrant farming. Informants say frankly that the opening up of new territory relieved a potentially dangerous situation in which young men could not acquire land. My own figures (Table 3) indicate that in shifting farming, compared with intensive homesteading, the yield per acre is reduced, but the fact that the area of the migrant farms averages five times larger means that the total production is substantially increased. With virgin soil readily available, at least for the present, it is possible for the Kofyar to economize on labour rather than on land.

Table 2—IGBO POPULATION, FALLOW PERIOD, AVERAGE ACREAGE (FROM MORGAN 1953)

Area	Population density	Length of fallow years	Average holding acres
Aba..........	over 1,000	3	3
Nvosi.........	436	5	4
Ndoki........	148	7	10 or more

Changes in settlement pattern, household organization, and land tenure do not arise independently. On the contrary, they appear to adapt the society more

Table 3—COMPARATIVE PRODUCTION OF KOFYAR HOMESTEAD AND MIGRANT BUSH FARMS

Type	No. in sample	Average acreage	Average production per acre (lb)				Average total production			
			Millet	Sorghum	Cowpeas	Totals	Millet	Sorghum	Cowpeas	Totals
Homestead farm (Intensive)	7	1.54	400	495	98	996	616	766	151	1,534
Migrant bush farm (Shifting)	10	7.75	368	305	74	747	2,855	2,366	572	5,793

closely to the needs of subsistence as prescribed by man/land relationships. A new set of social institutions is not thought inherently desirable nor do changes confer prestige. In fact, the stated norms for settlement, household composition, and land tenure are generally those of the traditional society, even while these are being superseded in practice. Departures from customary practice may be judged unfortunate but necessary by the people involved, or they may merely be overlooked. To the best of my knowledge, the Kofyar are not even aware of the numerical increase of extended families among migrant farmers because it is only evident in statistical analysis. I can find no intelligible sequence of events beginning as an independent change in land tenure or settlement pattern and, during the short span of fifty years, resulting in an altered population and agriculture. The priority of demographic change, however, has been noted by other observers.

As early as 1914 the unusual example of the Igbo having farmland as private property in Ibuzo was connected with the size of population in the area and the restriction of land by neighbouring settlements (Thomas 1914: 147). Evidence has since accumulated that dense population is correlated with a dispersion of compounds (Forde and Jones 1950: 17), an emphasis on the small family household as the basic unit of production (Green 1964: 34), and ownership of land by successively smaller kin groups — the smallest being the individual (Field 1945; Jones 1949; Chubb 1961; Morgan 1953). Udo (1965: Figures 6, 7) has shown not only a close contemporary correlation between regions of heavy rural population and dispersed settlement but also the change through time as single villages have spread through farmland and vacant bush to create wide areas of continuous compounds. He has suggested that a close grouping of habitations had formerly been necessary both for defence and for smooth operation of the block-farming system. Though different fields were used from one year to the next, they were approximately equidistant from the village centre. With pacification of the Eastern Region in 1901 and abolition of the slave trade, nucleated settlements were no longer needed for protection. The steady growth in population forced conversion of bush fallow land to homesites and permanently farmed gardens.

The traditional Kofyar settlement pattern of contiguous dispersed homesteads also coexists with local population densities of up to 1,200 people per square mile. The Kofyar found that living in the midst of thickly interplanted sorghum, millet, cowpeas, and vegetables enabled them to care for their crops daily, to protect them continuously from pests, and to apply more easily and efficiently the composted manure so necessary for sustained yields. The nearer the field the less time is wasted in travel and transport to the scene of work. The group naturally equipped to provide such continuous attention is the small, co-resident household. The addition of other labourers to the family group brings no corresponding increase in production because of the limited area of the homestead farm (Netting 1968). When the task is one of rapidly cultivating a much larger bush farm, however, the Kofyar mobilize a larger work group by exchanging labour or providing a beer feast for volunteers. The premium is on speed so that the entire field may be planted or weeded at the opportune moment of the growing season. Competition and social recognition stimulate individual effort. A larger resident labour force, such as the extended family, offers a much higher production potential when applied to the extensive bush fields of the plain. Such a group can also carry on simultaneous operations in widely separated locations. There is little comparable information on work groups among the Igbo except for references to communal bush clearing by a kin group (Forde 1946: 65) and descriptions of the breakdown of large traditional compounds into independent, self-contained units of one man and his immediate family (Udo 1965: 62). Morgan's (1953) estimate of family size in crowded areas is 7, which is not far from the Kofyar average of 5.3. I suspect that the differential effectiveness of differently constituted work groups under varying agricultural régimes is again influential in family size.

Perhaps the least questionable relationship of social facts to the subsistence system is found in matters of land tenure. Efforts to secure the use of an adequate supply of land obviously increase in direct proportion to the scarcity of land. If the village or kin group can no longer meet the needs of its individual members, they will attempt to obtain land which is not subject to group control or further subdivision. Among the Igbo, so-called houseland with residential structures and gardens was always considered a permanent possession of the occupants and their heirs. The conversion of bush fallow into houseland meant that it passed automatically from communal to individual tenure (Jones 1945, 1949: 314). The reorientation was not secured by an alteration in the rules applying to land but by a change in the *use* to which the greater proportion of the land was put. Where land is scarce and a frequent source of dispute, the residential occupation of a plot is a practical means of asserting exclusive individual control over it (Udo 1965). The resulting pattern is one of small, dispersed, individually owned homesteads, permanently farmed by intensive methods. As Chubb (1961: 14) notes, "In the main, it may be said that availability of land determines the type of tenure and it is where the pressure is greatest that the few remaining areas of common land are most rapidly decreasing." Thus, dispersed settlement and individual tenure grow out of the same circumstances. Compound gardens like Kofyar homestead fields are continuously productive and bear economic trees which yield regularly over a long period of time. Improvements to land, house, and orchard also represent an investment of labour whose rewards would not willingly be foregone. When rights in land become concentrated in the hands of individuals, there must also be a development of mechanisms for reassigning land temporarily or permanently to fit the needs of a changing population. Thus both traditional Kofyar society and the emerging Igbo intensive farmers have allowed renting, pledging, and conditional selling of land. But when plentiful land permits bush fallowing, the individual requires only rights of usufruct. Fields that must rest for a number of years are worth very little during the period of bush regeneration. They need only be protected from encroachment by neighbouring communities and redivided when their fertility has been restored. These tasks can be handled most effectively by a corporate group, a village, or a lineage. Where population density is low, this system of communal tenure is still practised by the Igbo, and it has been readily assimilated by the Koyfar in their new migrant holdings.

Summary, Qualifications, Implications

In summary, we may say that population pressure appears to be the critical variable, the engine which sets in motion adaptive changes in a set of related technological and social variables among subsistence cultivators. This conclusion receives support from a recent book by Boserup (1965) subtitled "The Economics of Agrarian Change under Population Pressure." Though the man/land relationship is central, we cannot say *a priori* just what the terms of the equation must be. The techniques of the farmers along with the physical or social restrictions of their land base must be considered. If an expanding population and/or a dwindling territory are rendering the returns from shifting agriculture insufficient to maintain a customary standard of living, and if additional land or alternative occupations are not available, cultivation will become increasingly intensive. To discern the operation of these factors, we may require edaphic, demographic, and nutritional studies as well as longitudinal data on land use and out-migration. My guess would be that under conditions of hand cultivation there is a 'tipping point' when rural population density reaches something between 200 and 500 persons per square mile (cf., Forde 1946: 105). In West Africa, the figure for the grain-growing savannah area may be at the lower end of this range, while that for the root croppers of the tropical forest would tend toward the upper limit.

Whenever it is felt, agrarian population pressure on resources leads to smaller plots and shortened fallows with the resulting adaptive value of artificial fertilization, soil conservation, high-yielding crops, and economic trees which maintain the land in permanent production. The application of these intensive practices in turn increases the selective advantage of dispersed settlement, small household productive units, and individual tenure. These social instrumentalities facilitate the necessary higher expenditure of labour by physical contiguity to the field, full mobilization of closely supervised, responsible workers in small groups, and exclusive enjoyment of the long-term returns on the heavy investment of time and effort.

Such a model as I have offered here invites application to comparable systems and other examples of agricultural change. It may be that it corresponds most closely to conditions in relatively self-sufficient communities with acephalous political institutions. But even a highly developed kingship with a market economy, specialists, and true cities may create a niche in which the same factors are relevant. Surrounding such Hausa cities as Kano in Northern Nigeria is a zone of very dense rural population practising intensive cultivation of grain and garden crops on manured land around their dispersed compounds (Grove

1961). They are typical peasants receiving protection and administration from the nearby city and providing garden truck and other produce for the urban market.

The interrelated factors that we have discussed do not, of course, exist in a vacuum, nor are their dynamics entirely mechanical. The advantages of dispersed settlement may be foregone, even with relatively high population density, when defensive needs become uppermost. The advantages of a larger community for building fortifications and military cooperation are self-evident. On the other hand, a trend toward shifting cultivation is not necessarily accompanied by nucleation. In the Kofyar case, enforced peace allows wide dispersion on the migrant bush farms. If a system of regular rotation of bush fallow land develops, there may be some advantage in the people settling together at a point equidistant from the various fields.

Though the comparison of Kofyar and Igbo cannot provide a set of axioms about ecological relationships and processes, I think it suggests a potentially fruitful approach to similar problems. Where we find intensive agriculture, we should look first for the relationship between population and arable land, remembering that people do not usually work harder than they have to. Land may be restricted by physical barriers, such as the arid strips hemming in the coastal valleys of Peru. Carneiro (1961) effectively contrasts the intensive agriculture there with the shifting cultivation of the wide-open Amazon lowlands. The most intensive and probably the most laborious farming system of Africa is found on Ukara Island in Lake Victoria. The island inhabitants made the best of a limited land base because it offered a natural refuge from mainland enemies (Allan 1965). Progressively more painstaking methods of wet rice cultivation have developed in response to phenomenal population density in Java (Geertz 1963) and more recently in Malaya (Rutherford 1966). It would be interesting to check some of these examples for correlated social variables.

If the factors we have examined are indeed functionally related in a causal sequence, there are obvious implications for planned change. Innovations may be more successfully introduced if they are offered when population pressure makes some change inevitable. Planners will no longer begin with a dependent variable, trying to increase agricultural production by first altering land tenure rules or insisting on high labour-input methods when abundant land makes their use uneconomical. When a certain critical density of population is reached under given agricultural conditions, a rapid change in farming techniques and an increase in human labour are possible. As an increasing population encroaches on available land, the producer uses his resources more efficiently, works harder, and becomes more receptive to innovations in crops and farming techniques (Jones 1943).

Though both Kofyar and Igbo cultures offer impressive evidence of adaptive potential, in neither case is the future assured. If the migrant Kofyar incorporate all vacant bush land in their new farms, fallows may be shortened to meet demands for both subsistence and cash. The savannah soils might be at least temporarily degraded. Though this danger could be met by a further cycle of agricultural intensification, perhaps the cost of creating an impoverished peasantry would be undesirable in human terms. Igbo population density has reached the point in some areas where no further intensification of agriculture is feasible. The land is over-farmed and food must be imported, while unemployment and seasonal migration of labour grow (Jones 1943). Long-range further adaptation in both cases will depend on new opportunities in the non-agricultural segments of the Nigerian national economy.

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POPULATION DISPERSAL AND LAND REDISTRIBUTION AMONG THE MARING OF NEW GUINEA[1]

BY

ROY A. RAPPAPORT

This paper describes the social and cultural means by which the Maring, a New Guinea people, disperse personnel over available land and redistribute land among local groups. After presenting some general environmental and ethnographic information, I shall first discuss processes occurring within the separate local groups and attempt to interpret some of the formal aspects of Maring social organization in terms of these processes.

The data upon which this paper is based were collected during fourteen months of field-work in 1962 and 1963 among a single local Maring group, the Tsembaga. Because of the limited size of the sample and because some important data are lacking, the interpretations in the first portion of this paper must be regarded as preliminary. It may be that some modification will become necessary when the results of field research of a more extensive nature, still being conducted among the Maring by A. P. Vayda, become available.

In the second portion of this paper I shall deal briefly with processes involving two or more local groups, incidently describing some of the ways in which religious practices are directly involved in the adjustment of the Maring to their environment.

The domain of the Maring is located in the Jimi and Simbai valleys in the Bismarck Range in the Western Highlands and Madang Districts of the Australian-administered trust territory of New Guinea. Linguistic and gross botanical evidence suggest that the Maring speaker's occupation of their present domain is more recent than the occupation by the linguistically related peoples of the 'true highlands' south of the Wahgi Divide; the Maring are the most northerly of the peoples speaking languages of the East New Highlands stock; the Maring area is more heavily forested than areas south of the Wahgi. On the northeast the area borders a vast stretch of unoccupied but potentially arable land, which remains under climax forest.

Tsembaga is one of about twenty local groups among which the 7,000 Maring speakers are distributed. These groups range in size from about 100 to 900 persons. Only recently has contact been made with them. The first government patrol to penetrate Tsembaga territory arrived in 1954; they were considered uncontrolled by the government until 1962; and they remain unmissionized. Earlier contact had been indirect. Steel axes first arrived over trade routes in the late 1930's but did not become common until the early 1950's. Dysentery and probably measles also came over the trade routes before the white man and helped to reduce the population from a previous level of 250–300 to the 204 persons in residence in 1963.

The Tsembaga, who live on the south wall of the Simbai Valley, are completely surrounded by other Maring-speaking groups. They are friendly with the groups across the Simbai River to the north and across the ridge of the Bismarck Range to the south, and with the Tuguma, the local group immediately to their east on the same valley wall. The territory immediately beyond their western border is held by the Kundagai, with whom the Tsembaga fought on four separate occasions, roughly between 1915 and 1953.

This spatial distribution of friends and enemies seems to be the typical Maring pattern. According to Vayda (pers. comm.), almost every Maring local group shares at least one border with an enemy, and in almost every case the antagonists dwell on the same side of the valley. Enmities between groups separated by the major rivers on the floors of the valleys, the Simbai and the Jimi, do occur, but they are rare and transitory. Enmities between groups separated by the mountain ridge do not occur at all. It is relevant here that the trade routes run generally across the grain of the land. From groups living across the river or over the ridge, the people obtain commodities that do not occur locally. The lands of these groups are not usually coveted, however, because it is difficult for people to exploit areas that are separated from their residences by major rivers or ridges. Conversely, it is not surprising that relations between groups living side by side on the same valley wall are frequently antagonistic. Adjacent groups do not depend upon each other for locally unobtainable commodities; their proximity increases the likelihood of friction, their lands being convenient to each other's residences.

Tsembaga territory, which is generally lower and more rugged than the 'true highlands,' rises, within a total surface area of 3.2 square miles, from 2,200 feet at the Simbai River on the north to 7,200 feet at the ridge crest to the south. With the exception of gardens, small remnants of climax forest, and a few patches of grass dominated by *Imperata cylindrica*, secondary forest covers the surface below 5,000 to 5,400 feet. Above this line the entire area remains under climax forest. Economic density, reckoned against areas in secondary forest and gardens, is 124 persons per square mile; reckoned against all potentially arable land it is about 97 persons per square mile. Rainfall reaches 150 inches per year.

The Tsembaga keep pigs and do some hunting, gathering, and collecting, but 99 per cent by weight of their daily diet in ordinary times comes from gardens slashed and burned in secondary forests, and from groves. Staples include a range of root crops, *Colocasia* and sweet potatoes being most important; yams, manioc, and *Xanthosoma* less so. Bananas are also raised. The crop inventory also includes a great variety of greens, some of which are rich in protein. Sugar cane and some tree crops, particularly *Pandanus conoideus*, are also important. New gardens are made every year, and cropping continues for 18 to 24 months after planting, after which the garden sites are abandoned to the already regenerating secondary forest.

All gardens are mixed; many of them contain all of the major root crops and greens, as well as sugar cane and bananas. Two named garden types are, however, distinguished by the crops which predominate in them. Taro-yam gardens, planted in the lower altitudes, were found to produce about 5,200,000 calories per acre during their harvesting lives, with 85 per cent of their yield being taken between 24 and 76 weeks after planting. These figures are estimated on the basis of daily harvest records kept on entire gardens for close to one year. Sugar-sweet potato gardens, usually planted between 4,400 and 5,400 feet, produce about 4,600,000 calories per acre with 91 per cent being harvested between 24 and 76 weeks after planting (Rappaport 1966: 69–70). I have estimated that about 315,000 calories per acre are expended in labour on taro-yam gardens, and about 290,000 calories per acre on sugar-sweet potato gardens (Rappaport 1966: 73). These energy ratios, approximately 17:1 and 16:1, compare favourably with figures reported for swiddening in Borneo, Africa, and Central America (Rappaport 1966: 425ff.).

Intake is high in comparison with the reported diets of other New Guinea populations. On the basis of daily consumption records kept for ten months for four households comprising 16 persons, I estimated the average daily intake of adult males to be approximately 2,600 calories and that of adult females, 2,200 calories (Rappaport 1966: 112). In this connection it should be mentioned that the Tsembaga are very small. Adult males average 4 feet 10½ inches in height and 101 pounds in weight. Adult females average 4 feet 6½ inches in height and 85 pounds in weight. Their protein status seems to be better than that of many other New Guinea populations, but marginal none the less.

Measurements of all gardens made during 1962 and some gardens made during 1963 indicate that between .15 and .19 acres are put into cultivation per capita per year to support the human population. Fallow periods vary on different sites from 5 to 45 years in length. Therefore only 30 to 50 of the approximately 1,000 acres in secondary forest are in cultivation at any time. Assuming that calories from domesticated sources are the limiting factor and that an unchanging age–sex structure exists within the population, I have estimated that Tsembaga territory could support 290 to 390 people without accelerating the cultivation–fallow cycle, and without cutting into climax forest (a source of many important items), if it were not for their pigs. Pigs are given a ration of tubers, an adult pig receiving about as much as a grown man. Taking into consideration pig husbandry practices, then, I have estimated the carrying capacity of Tsembaga territory to fall between 270 and 320 people, or approximately 170 to 200 people per square mile (Rappaport 1966: 132ff.).

The Tsembaga and other Maring are highly egalitarian. There are no hereditary or elected chiefs or other regular political offices. Some respected men who seem to initiate community action more frequently than others are said to be 'big men,' but they are unable to coerce or command the support of others in anywhere near the degree that Oliver (1955: 422ff.) has reported for 'big men' among the Siuai.

The Tsembaga are organized into five named putatively patrilineal clans. These clans, the most inclusive groups claiming common ancestry, range in size from 15 to 78 resident members, including in-married wives. Three of the five clans are divided into smaller named segments, or subclans, ranging in resident membership from 7 to 37 persons. The two smallest clans are unsegmented.

The settlement pattern is fluctuating. At the time of pig festivals, which occur cyclically at intervals of ten to twenty years, all or almost all Tsembaga residences are concentrated in the general vicinity of a dance ground. At other times, settlements are likely to be of clan or subclan size (although not strictly of clan or subclan composition), with some people living in isolated homesteads.

For the purposes of defence and certain subsistence activities, the Tsembaga form a single territorial unit. Any Tsembaga may hunt, trap, or gather wild plants (important not so much for food but for fuel and building and gardening materials) from any part of the jointly claimed territory. These rights are exclusive to the Tsembaga. That is, members of other local groups do not enjoy such rights in the non-domesticated resources to be found on Tsembaga territory, nor do the Tsembaga have such rights on the territories of other groups.

Although the entire territory is open to the hunting and gathering activities of all Tsembaga, smaller areas are claimed by less inclusive groups. Ideally these should be clans, but the division is tripartite even though Tsembaga is composed of five clans. The entire territory is divided into three adjacent strips running from ridge top to river; two of the subterritories are separately claimed by two of the clans, and the third is claimed jointly by the three remaining clans.

The three subterritorial groups are exogamous, but the Tsembaga as a whole are not. Statistically and explicitly the men prefer to marry women of proximate origin. Forty-four per cent of the fifty wives and widows residing on Tsembaga territory in 1963 were Tsembaga in origin and an additional 22 per cent came from the Tuguma immediately to the east. The remaining 34 per cent came from nine other local groups occupying more distant territories.

The three subterritories are subdivided into smaller areas claimed by smaller constituent units, either clans or subclans, each of which holds a number of non-contiguous tracts scattered throughout the full altitudinal range.

Males have rights in the entire estate in garden land of the subterritorial group by virtue of clan and subclan membership. Although individual men claim title through patrilineal inheritance to particular sites, adjustments of inequities are readily made. If a man is short of inherited sites, he simply asks a better endowed member of his subclan for land, and a transfer in perpetuity always seems to be made. An individual's title to garden sites thus amounts to no more than stewardship for his subclan. Similarly, the title of a subclan in a tract of land may be regarded as stewardship for the entire clan. If a subclan is short of land, its individual members may ask for and receive grants of land from members of better endowed subclans, and these subclans may even grant it entire tracts.

In the case of the subterritorial cluster of three clans, adjustments of inequities in land holdings are also readily made between members of these clans. That is, so far as access to land is concerned, these three clans relate to each other as do the subclans or a single clan. They are, at present, antigamous in reference to each other, but they are, or believe themselves to be, related by connections through females in ascending generations when intermarriage among them is either known or believed by them to have taken place. That is, the members of the three separate clans regard each other as cognates, even though actual connections frequently cannot be traced. It could be argued that although this multi-clan structure remains *de jure* a cluster of three agnatic descent groups, since the principle of filiation is patrilineal connection to one of the three constituent clans, it may be regarded as, *de facto*, a corporate cognatic descent group since its combined membership has rights in a common estate in land. I shall therefore refer to it as a 'cognatic cluster.'

Regardless, however, of whether rights to land are based upon agnation or cognation, within the three subterritorial groups, serious differences in land holding are ameliorated as quickly as they become apparent. It may be suggested that the ease of transfer displayed by Tsembaga land tenure practices is particularly advantageous among societies organized into small groups, for small groups are highly vulnerable to sudden and drastic demographic fluctuations, independent of those experienced by their neighbours. The transfers not only avoid social and economic inequities but protect the environment from the over-exploitation of some areas while other areas remain under-exploited.

The subterritorial groups are themselves small and thus are highly subject to independent demographic fluctuations. Grants in land are, however, frequently made by members of one subterritorial group to members of another. From a sample of 381 gardens planted in 1961, 1962, and 1963, 94, or 24.7 per cent, were worked by men who did not belong to the subterritorial group that claimed the land. It would seem that, despite the claims of less inclusive groups to particular areas, all Tsembaga do, in fact, have access to garden land throughout the entire territory. Since this is the case, and since all Tsembaga enjoy joint or exclusive rights to the non-domesticated resources found on their territory, it is, I think, correct to regard the Tsembaga as a population in the ecological sense: one of the components in a system of trophic exchanges taking place within a bounded area, in this case Tsembaga territory. I shall, accordingly, refer to the Tsembaga and similar units as 'local populations.'

There are, however, differences in the nature and pattern of land transfers between members of constituent units of the same subterritorial group and between members of separate subterritorial groups. In contrast to transfers between members of separate constituent units of the same subterritorial group, which were in 28, or 85 per cent, of the cases grants in perpetuity, only 35 per cent of the transfers between members of separate subterritorial groups were grants in perpetuity. The remainder were usufructory grants made, in most cases, to cognates or to wife's brothers. Of 33 grants in perpetuity made by members of one subterritorial group to members of another, 26, or 79 per cent, were made by men to *husbands of female agnates*. Such affinal grants are first made when it becomes apparent that a marriage will be an enduring one, but in most cases additional grants are made throughout the lives of the principals. These grants, which confer rights only in specified sites and not in the entire estate of the grantor, do not require the grantee to take up uxorilocal residence, and he seldom does. In most cases, sites received from a wife's agnates only supplement the lands to which a man has rights by virtue of patrifiliation, lands which he usually continues to rely upon most heavily. Whether or not he needs lands received through affinal grants, he is likely to use them, for he is under pressure to do so from his wife and his affines. His wife is likely to wish to make some of her gardens on the land of her agnates so that she may visit them frequently. On the other hand, a woman's agnates encourage her husband to take up these lands for less sentimental reasons. From their point of view, indeed, the affinal grant provides a means for retaining access to a portion of the labour of female agnates after their marriages. The sites transferred to a woman's husband are usually adjacent to those being cultivated by her agnates; thus, without great inconvenience a woman can assist an unmarried brother or a widowed father with his garden, and she often does.

In light of the considerations involved in cultivating land received from wives' agnates, it is not surprising that these sites, although said to be transferred in perpetuity, often revert to the grantors after one generation. The pressures that induce a man to cultivate land received from his wife's natal group are absent from his relationship with his mother's natal group. If, however, the lands his father has received are convenient to a man's residence, he is likely to continue to use them. Therefore, affinal transfers between adjacent subterritorial groups are likely to endure transgenerationally. The result is that the garden sites claimed by members of adjacent subterritorial groups become intermingled. Such intermingling has proceeded far among the Tsembaga, particularly between the cognatic cluster, which occupies the central subterritory, and the clan which occupies the subterritory to its west. Indeed, the two subterritories seem to be becoming one. People say that the land of the two groups is one, and many younger men claim to be ignorant of the location of the boundary. Older men are in agreement about its location but invariably state that this boundary no longer has great relevance.

In connection with the gradual obliteration of this subterritorial boundary, it is interesting to note that four of the land transfers in perpetuity between the two subterritorial groups were made not on the basis of current affinal connections but on the basis of affinal connections in ascending generations, that is, cognatic connections. The number is small, but I believe that it not only marks a trend but illuminates the processes of group formation. Although evidence is insufficient, it may be suggested that the cognatic cluster and the more inclusive structure that subsumes both the cognatic cluster and the clan occupying the subterritory immediately to its west represent two points in a continuous process of land amalgamation. Through intermarriage, and then by grants in perpetuity through affinal connections, the land of adjacent groups becomes intermingled. As neither a man nor his subclan 'brothers' should take a wife from his mother's natal clan, there is partial antigamy between the groups in alternate generations; but the intermingling is maintained because sons tend to maintain rights ceded to their fathers by their mothers' brothers in sites near their own patrilocal residences. Thus, they remain in constant contact with their mothers' natal groups and occasionally receive new grants of land on the basis of their maternal connections. More intermarriage elaborates the web of cognatic relationships for descending generations and further intermingles the garden sites of the two groups. People come to talk of the subterritories of the two groups as being one territory, and the border becomes indistinct. Eventually members of each group can trace cognatic connections to almost everyone in the other group. Cognatic connections replace marriage as the preferred means for acquiring rights in land, and intermarriage ceases. What may be called an 'affinal cluster' (two or more clans whose garden lands are becoming intermingled through affinal transfers) has become a cognatic cluster. Kinship terminology would facilitate this process. Terms are Iroquois on Ego's generational level and bifurcate merging in the first ascending generation, but generational in the first descending and second ascending generations. Terminological distinctions between members of one's own exogamous group and groups with which it has intermarried are thus obliterated in two generations. Whereas cross-cousins refer to each other by special cross-cousin terms, the children of cross-cousins refer to each other by the terms for brother and sister, although they may marry. This usage may be a formal concession to processual possibilities, for it permits, without terminological disruption, the fusion of separate intermarrying groups into a single corporate cognatic group or cognatic cluster.

It may be that cognatic clusters become putative agnatic descent groups with the passage of time. Since a subterritorial clan cluster is functionally equivalent to a subterritorial clan, such a transformation requires only a change in 'charter.' Once again the kinship terminology would facilitate such a change, since it masks the distinction between cognates and agnates after two generations. Moreover, ideological consistency would call for such a transformation, for according to the Tsembaga, territoriality is ideally associated with agnation. It might be expected, *a priori*, that when knowledge of actual connections fades, agnatic connections between the constituent (clan) units of the (cognatic) cluster will be assumed. This expectation is not, however, supported by the cognatic cluster found among the Tsembaga. Marriage connections between one of the clans and the other two are recent and therefore are well remembered. In the case of the other two, however, which have been together for a much longer time, specific marriage connections are no longer remembered, but the two clans retain their separate agnatic identities.

So far, I have only spoken of land amalgamation leading to group fusion. That this process is sometimes reversed may be illustrated by the easternmost of the three subterritorial groups. At the time of my field-work this group consisted of a single exogamous clan, but in the past, members of one of its three subclans intermarried with members of the other two. Informants say that when the clan was far more populous and occupied less land than it does at present, one of the subclans moved its houses away from those of the other two and began to concentrate its gardening activities in the vicinity of its settlement. Eventually its lands became separate, and intermarriage with the other two subclans commenced, although common ancestry was still acknowledged.

What may have induced the removal of the residences, if such occurred, is beyond the memories of surviving clan members, and I will refrain from speculating about the mechanisms that may have effected land segregation. All that is certain is that intermarriage between what are presently subclans of a single clan (Merkai) did take place (both partners of one such marriage were alive in 1963) and that in former times a larger population did occupy a smaller area. There is, indeed, strong reason to believe that the Merkai were approaching or exceeding the carrying capacity of their land during at least part of the period when intermarriage was taking place among what are presently its subclans.

While data based upon my own observations or the observations of living informants are lacking, I believe that the informants' statements, which at least temporally relate the transformation of antigamous subclans into intermarrying clans with population pressure, should at least be taken to reflect a processual possibility. This belief is reinforced by Meggitt's (1965:16) observation of a similar process among the Enga people of the Western Highlands.

In this connection it may be noted that although the practice of conveying land ownership between members of different subclans of the same clan and between members of different clans, or subterritorial groups, may disperse population evenly over available land, it cannot alleviate a general land shortage. As critical density is approached or exceeded, it may be that land-claiming units at all levels discourage the cultivation of their lands by members of coordinate units. An increasing emphasis on the primary principle of filiation, in this case agnation, as the basis for claims to land might then be expected, as Meggitt (1965: 260ff.) has proposed. Intra-clan marriage accompanied by affinal land transfers might well be part of the associated strategy. Such marriages transform clan agnates into affines (affinal, rather than agnatic terms of reference and address are used), and affinal transfers of land may replace the recipients' residual rights as agnates in the subclan estate of the donors with the more limited rights of affines in specified garden sites, while leaving intact or ambiguous the donor's agnatic rights in the lands of the recipient. Sufficient data to support adequately or refute this *post hoc* interpretation are lacking but may become available as a result of Vayda's field-work.

In summary, it may be tentatively suggested that when population densities are low and considerable distances separate neighbouring groups, there is little permanent intermingling of garden land, only because land acquired by virtue of membership in an agnatic group is convenient to a man's residence and other lands are not. With moderate to moderately high densities and reduced distances between neighbouring groups, intermingling increases, for conveniently located lands may be obtained through affinal and then cognatic connections. As lands become intermingled, previously distinct social groups fuse, and in doing so they become antigamous. As critical density is approached, the primary principle of filiation is emphasized as the basis for rights in land: garden intermingling is inhibited, and social groups are likely to fission or split, in which case the members may begin to intermarry. This interpretation tends to support the argument of Meggitt (1965: 260ff.) concerning the relationship between agnation and population pressure in the New Guinea Highlands, and to reconcile some of the differences between him and Brown and Brookfield (1959) and Brown (1962).

So far, I have discussed processes occurring within Tsembaga. I have not discussed the processes by which the five agnatic descent groups emerged as a single distinct unit from a population composed of many agnatic descent groups, dispersed more or less continuously over the wider geographical area. Clan histories indicate that warfare played an important part in this development. Tsembaga informants say that approximately fifty years before my field-work, the gardens of the westernmost Tsembaga clan (Kamungagai) and the gardens of the Kundagai to their west were intermingled; recent litigation supports their memory. In a fight over a woman, however, one Kundagai was killed. Warfare immediately followed, with each of the battling clans receiving the support of clans located behind it. Within days of the termination of this round of warfare, trouble developed between the Merkai, the easternmost clan of what was to become Tsembaga, and the Dimbagai–Yimyagai, immediately to the east of them. In the subsequent fighting the Dimbagai–Yimyagai were driven from their territory, which the Merkai eventually annexed.

It may be noted that the garden lands of the Merkai and the Dimbagai–Yimyagai were very much intermingled at the time of the fight, and that intermarriages had taken place more frequently between these two groups than among other groups. In line with earlier suggestions, the two groups may have been in the early stages of fusion when fighting broke out between them.

The circumstances surrounding this fight suggest that population pressure was its underlying cause. Precipitating events, such as the stealing of sugar followed by a superficial wounding of the culprit, would not usually have led to warfare. More important, rough estimates indicate that at the time of the fight the Merkai had exceeded the carrying capacity of their territory; this conclusion may be supported by information concerning the widespread violation of the patrilocal residence rule. Informants say that when fighting broke out nine Merkai men had been living and gardening uxori-, matri-, or sorori-locally on Dimbagai–Yimyagai territory, while only one Dimbagai–Yimyagai man was living on Merkai territory. The widespread nature and asymmetrical pattern of these violations suggest, and informants assert, that the patrimonial land of the Merkai was in short supply and that the nine Merkai men were living on Dimbagai–Yimyagai territory because all or most of their garden sites were there.

Thus, warfare, which seems to have been the result of increasing population pressure, defined the borders of Tsembaga territory, and the joint participation in warfare of the Tsembaga as a unit distinguished them from other units. This *de facto* association of previously independent groups became a *de jure* structure through the performance of the rituals which follow the successful termination of the fighting.

The fundamental corporate interest of this structure, the defence of the combined but separately claimed lands, had implications for the access by all Tsembaga to the combined lands. In the face of a common enemy it became necessary to maintain a common strength. As the upper limits of population size are set by the capacity of the territory, so lower limits are set by the strength of the enemy. This is illustrated in recent history. In 1953, the Tsembaga were routed by the Kundagai, their enemies to the west, and were returned to their territory by the Australian government in 1957. Out of fear of the Kundagai, however, all Tsembaga initially built their houses and made their gardens on the subterritory of the Merkai, the easternmost clan. While cognatic and affinal connections served as the basis of the land conveyances by the Merkai to the others, I feel that these connections were hardly more than *post-hoc* rationalizations. I believe that the overriding consideration in granting house sites and garden land was not obligation to kinsmen, but the desire to reassemble a group large enough to defend itself. That the members of the other two subterritorial groups did not have, *de jure*, corporate rights in Merkai lands made little difference, for it was in the corporate interests of the Merkai to grant them garden lands sufficient to their needs. The demands of common defence may thus provide a motive for transferring land ownership among members of the several subterritorial groups, which together comprise a local population.

I shall now turn briefly to a consideration of the mechanisms by which people are redistributed and land is divided among two or more local populations.

Ownership of land tracts is sometimes transferred peacefully from one territorial group to another. For example, previous to and during the period of my field-work, the Tsembaga were gradually taking possession of an area north of the Simbai River that had previously belonged to a local population called the Kekai. As I reconstruct the process from informants' statements, the first Tsembaga to cultivate these lands were men with kin among the Kekai. They doubtless thought of the area as part of Kekai territory on which they, as cognates or affines, occasionally received rights in limited tracts. As the Kekai declined in numbers, however, the Tsembaga apparently assumed corporate rights in the entire area. The transformation from individual rights in specific sites to corporate rights in the general area was almost complete by 1963. By that time Tsembaga men could point to borders between tracts claimed by various Tsembaga subclans, although some men continued to cite their connections to the Kekai, rather than their agnatic filiation, as warrant for the use of these lands.

In contrast to the gradual displacement of one territorial group by another, as exemplified by the Tsembaga–Kekai case, there have been instances in which members of one local population have invited constituent units of another local population to join them on their land. Without demographic data no firm statements can be made. However, informants' accounts do strongly suggest that such invitations were issued by groups that had more land than they needed for their support and fewer men than they required for their defence. I know of four instances of such land grants in Tuguma–Tsembaga history; one will serve as an example.

Informants say that several generations ago an internal fight erupted within a clan called Kumom, which occupied a territory within the area later to become the territory of the Tuguma local population. The fight seriously weakened the Kumom. Although only one person was killed, one faction emigrated to the Jimi Valley after the fight. Those who remained could not look forward to the quick regeneration of their numbers through their own natural increase, for they and their children were few in number. Citing these facts and the further fact that they had more land than they required, they invited the Kungagai clan, who at that time occupied a territory adjacent to the Merkai in the eastern portion of what later became the territory of the Tsembaga local population, to join them on their land. There is reason to believe that the Kungagai, like their neighbours the Merkai, were approaching critical density at the time, and informants say, moreover, that they were racked by internal dissension. Most of them accepted the invitation, and some Merkai joined them in the move. In 1963 the Kungagai were an important Tuguma clan; the Kumom had become extinct.

I have mentioned in passing that one of the Tsembaga clans, the Merkai, annexed lands from which they had previously driven another group. Although the Tsembaga and other Maring say that they do not go to war to win land, warfare was frequent previous to the imposition of government-enforced peace in 1958 and often resulted in the redistribution of land and personnel among local populations.

Warfare was not continuous, nor was its eruption unpredictable. An elaborate and prolonged cycle of rituals regulated its occurrence, and the sacred conventions of this cycle defined the circumstances under which land previously occupied by an enemy could be annexed. Since I have discussed this ritual cycle in detail elsewhere, I will describe it only briefly here.

Some of the rituals with which I am concerned are performed by a local population as a concomitant of warfare; others, taking place over a protracted period (ten or fifteen years is not unusual), follow the termination of hostilities. Opponents in warfare invariably occupy adjacent territories, in almost all cases on the same valley wall. After hostilities have erupted, each side performs certain rituals which place its antagonists in the formal category of 'enemy,' and a number of tabus come into effect.

Fighting, which can last sporadically for weeks, usually concludes in a truce. Occasionally, however, it terminates in the decisive defeat and rout of one of the parties; survivors of this rout take refuge with kinsmen elsewhere. In such instances the victors make it as difficult as possible for the defeated to return to their land by laying waste their groves and gardens, slaughtering their pigs, burning their houses, and desecrating their sacred places. They do not, however, immediately annex the territory of enemies, for even if the enemy has taken flight, the spirits of his ancestors remain to guard the land against trespassers. The victors, therefore, retire to their own territory after spreading as much ruin as ingenuity suggests and enthusiasm and energy sustain.

With the termination of hostilities, a successful antagonist (i.e., a local population which has not been driven off its land) performs rituals called 'planting the *rumbim*.' Each of the subterritorial groups comprising the local population performs this ritual separately but concurrently. As the sacred shrub, *rumbim* (*Cordyline fruticosa*) is planted in the ground, each man places his hand upon it, thus symbolizing his attachment to the land. The ancestors are thanked for their assistance in the fight and told that out of gratitude the people will now give them all their available pigs. They are told, however, that the pigs are few (whether, in fact, they are few or many), but that some time in the future, when there are many pigs, the *rumbim* will be uprooted and a festival staged, during which they will be given pigs in quantities commensurate with their assistance. Until that time the *rumbim* is to remain in the ground. The *rumbim* thus signifies an outstanding debt to the ancestors, as well as attachment to the territory.

All adult and adolescent pigs are killed at this time; only juveniles are excluded from the slaughter. Although the animals are dedicated to the ancestors, they are consumed by the living. Some of the flesh is retained by the locals, but most of it is distributed to their allies.

Some of the tabus assumed during the fighting are abrogated by this ritual, but others remain and certain new ones come into effect. The most important in the present context are those related to the enemy, to enemy territory, and to warfare. One may not touch, talk to, or even look at a member of the enemy group, nor set foot on enemy territory. Moreover, a group may not attack another while its own *rumbim* remains in the ground, for it has not yet discharged its obligations to its ancestors and allies for their assistance in the last fight; and until the obligations are discharged their further assistance will not be forthcoming. A kind of 'Truce of God' thus prevails until the *rumbim* is uprooted, and a *kaiko*, or pig festival, is completed.

The frequency of warfare is therefore a function of the demography of pig herds, for to uproot the *rumbim* and repay the ancestors requires sufficient pigs. The questions of what constitutes sufficient pigs and how long it takes to acquire them are raised.

The Tsembaga say that if a place is 'good' it can take as little as five years to acquire sufficient pigs, but if a place is 'bad' it can take ten years or longer. Apart from warfare and festival occasions, pigs are usually killed only as sacrifices in connection with such misfortunes as illness and injury. A bad place is one in which such events are frequent, and where, consequently, the demands for the ritual slaughter of pigs are high. Conversely, in a good place the increase of the pig herd exceeds continuing ritual demands, and the herd grows rapidly. Sooner or later, however, the herd becomes a burden. Substandard tubers incidently obtained in the course of harvesting for humans become insufficient to feed the pigs and additional acreage has to be used for growing tubers to support the animals. The additional land and labour involved is substantial. Just previous to the festival of 1962–63 the Tsembaga pig herd, which numbered at the time 169 animals, was receiving 54 per cent of all sweet potatoes and 82 per cent of all manioc harvested. These comprised by weight 35.9 per cent of the total root crop harvested. This figure squares nicely with an independent measurement of land under cultivation before the festival and after, when the herd was reduced to sixty juveniles. There was 36.1 per cent more land in cultivation before than after.

I have estimated, on the basis of acreage yield and energy expenditure figures, that about 45,000 calories per year are spent by humans in caring for one pig of 120- to 150-pound size; and it is upon the women that most of the burden falls, for they plant, cultivate, and harvest the root crops (Rappaport 1966: 261). Women have an intake of about 2,200 calories a day, of which at least 950 calories must be allowed for basal metabolism. Since a woman must garden and cook for her family and care for her children, it is obvious that no woman can tend many pigs. Only a few had as many as four in their possession at the commencement of the Tsembaga pig festival in 1962, and informants' statements that the agitation to uproot the *rumbim* and slaughter the pigs started with the wives of owners of many pigs are not surprising.

Furthermore, as the number of pigs increases, so does the number of invasions of gardens by pigs. Such events are often attended by fights, shootings, and residential dispersal. A large pig population, thus, is not only a burden and a socially disruptive nuisance but also a competitor with the human population for garden crops.

It is when the labour demands or the depredations of pigs, or both, become intolerable to enough people to effect a consensus that the decision to uproot the *rumbim* is reached. To put this in ecological terms, there are sufficient pigs to stage a festival and repay the ancestors when the relationship of pigs to people changes from one of mutualism to one of parasitism or competition.

The first of the major rituals which comprise the festival demarcates the boundaries of the territory. If the enemy continues to occupy its territory, ritually prepared stakes are planted at the boundary that existed previous to the fight. If, on the other hand, the enemy is routed and does not return, the stakes are planted at a new boundary which encompasses areas that the enemy previously occupied. We may, thus, state here a rule of land redistribution in terms of the ritual cycle: *If one of a pair of antagonists is able to uproot its* rumbim *before its opponents can plant theirs, it may occupy the latter's territory.* Tsembaga, who say that they never disseize others of the land, are untroubled by this, for they say that the annexed area is not enemy land but vacant land since no *rumbim* was planted on it after the last fight. It is assumed that by the time stakes are planted even the ancestors of the enemy have departed. The living descendants of the enemy, who have taken refuge with other groups, are likely to have had occasion to sacrifice pigs to their ancestors; and the spirits, it is assumed, have taken up residence where they are being fed. The ancestors of the vanquished groups thus relinquish their guardianship over the territory, making it available to the victors. Meanwhile, their living descendants eventually acquire membership in the groups with which they have taken refuge. Sooner or later the groups with which they have taken up residence go to war. Upon returning, they plant *rumbim*, and the refugee men, as co-residents and co-belligerents, participate. By this act their *de facto* membership in the group is converted to *de jure* membership, for their connection to the land is validated in a public ritual. Thus, a rule of population redistribution may also be stated in terms of ritual cycles: *A man becomes a member of a local population by participating with it in the planting of* rumbim.

The uprooting of the *rumbim* follows shortly after the demarcation of the boundary. It is the occasion of the first major pig slaughter of the festival, which continues for over a year, during which time allies of the last fight and other friendly groups are occasionally entertained at all-night dances. The festival terminates in a massive pig slaughter. All the pigs are dedicated to the ancestors, but most of the resulting pork is distributed to allies.

After the termination of its festival the local population would be free, were it not for the government presence, to attack the enemy again, secure in the knowledge that support from allies and ancestors would be forthcoming, because debts to them have been discharged. They have been given sufficient pork.

It may be suggested that to accumulate sufficient pigs, a local population must utilize, to the limits of its ability, the territory that it already claims. Thus, the ritual mechanism which I have outlined here requires a local population to demonstrate ecological competence within its own territory before it initiates actions which can result in the annexation of more territory at the expense of other local populations. Thus, the occurrence of warfare is limited to frequencies that do not endanger the survival of the Maring population as a whole, but that do permit ecologically competent groups to expand.

Concerning social organization in New Guinea, Vayda and Cook have suggested that "it is probably more rewarding for social anthropologists . . . to focus upon . . . processes than to attempt to devise elaborate social typologies applicable to all the structural variations encountered on a single time plane" (1964: 802). In the first portion of this paper I attempted to illuminate some of the structural variations found at a particular time in terms of processes of population dispersal and land redistribution within the local population.

In the second portion of the paper I dealt with the processes through which land and personnel are redistributed among two or more local populations and tried to show how ritual is directly involved in these processes.

Although attention has been focused upon cultural phenomena, particularly upon aspects of social organization and religious behaviour, the conceptual framework underlying this paper has been that of 'general' rather than 'cultural' ecology. The Tsembaga have been regarded as a population in the ecologists' sense of the term: an aggregate of organisms having in common certain distinctive means whereby they maintain a set of shared trophic relations with other living and non-living components of the ecosystem in which they exist together. The boundaries of this ecosystem have been implicitly drawn at the borders of Tsembaga territory, for an ecosystem is a systemic web of material exchanges, and only the Tsembaga enter into material exchanges with the populations of other species inhabiting this area. At the same time, the Tsembaga have been regarded as a subunit in a larger unit, a regional population, composed of many local human populations similar to themselves. The model is not unlike one which would be appropriate to the analysis of the ecology and social organization of many other species. Among most other species of social animals, local groups are independent as far as subsistence is concerned, but they relate to each other in such matters as the exchange of personnel and genetic materials and the adjustment of home range and territorial borders.

Within this conceptual framework, aspects of Maring social structure and ritual behaviour have been regarded as nothing more nor less than parts of the distinctive means by which particular populations of one species maintain themselves in their environments. It is not claimed that this procedure yields all the elucidation that Maring social organization and ritual behaviour invites or requires. It can be stated, however, that a general ecological frame of reference can add an important dimension to our understanding of specifically human phenomena.

NOTE

1. This paper was delivered in 1966, and much of the material discussed here was later included in Roy Rappaport, *Pigs for the Ancestors*, New Haven, Yale University Press 1968. For a fuller discussion of Maring social organization as well as for a detailed discussion of Maring ecological relations and their regulation, the reader is referred to that work. The data upon which this paper is based were collected during field-work supported by the National Science Foundation and the National Institutes of Health.

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PART II:

CORRELATIONS OF EXPLOITATIVE AND SETTLEMENT PATTERNS

BY

GEORGE PETER MURDOCK

It has long been recognized that the form, size, and fixity of human settlements bear a definite relationship to the modes of exploiting the natural environment to provide subsistence. Hunting and gathering peoples, as is well known, tend to live in small nomadic bands, whereas those who depend on agriculture clearly tend to live a sedentary life in local groups of considerably greater magnitude. But what are the social concomitants of such alternative exploitative patterns as fishing and pastoralism? And since most societies practise a number of different subsistence activities conjointly, how are we to weigh the relative influence of each on the forms which settlement assumes? The riches of ethnography, if properly assessed, should be able to supply the answers to these and related questions.

To tap these resources, however, we require an adequate method of selecting the societies to be compared — one that is in complete accord with both ethnological theory and ethnographic realities. We need a large number of cases so that the results may be quantified. They must be distributed equably over the earth with a minimum of geographical gaps and no under-representation of particular areas or levels of cultural complexity, and the cases must be as independent as possible. This means, first, that no more than one case should be selected from any group of closely related societies, however widely dispersed they may be; and second, that no two chosen societies should be so close to each other geographically as to raise the presumption that their cultures have been significantly influenced by recent diffusion.

I have elsewhere (Murdock 1966) suggested a method for drawing such a world sample. Using this method, with some modifications to suit the particular problem in mind, I have obtained data on modes of subsistence and settlement patterns from 322 distinct cultures. Their geographical distribution is as follows: 50 from sub-Saharan Africa, 42 from the Circum-Mediterranean region (North Africa, Europe, and the Near East), 62 from East Eurasia, 59 from the Insular Pacific, 54 from indigenous North America, and 55 from South America. The data were obtained in most instances from the "Ethnographic Atlas" in *Ethnology* (Murdock *et al.* 1962–66), but, to cover areas not represented in the Atlas, data were drawn from other sources on 23 additional cultures.

To assure relative immunity from the influence of recent diffusion, no two societies were chosen whose geographical centres were not more than 200 miles apart — a distance readily measured by a separation of at least three degrees of latitude or of longitude (at the Equator, proportionately more at higher latitudes). Linguistic relationships were used to reveal a common origin in the case of more widely separated groups. Thus, for example, the sample includes only one society whose language is English and only one whose language is Mandarin Chinese.

The sample is not a randomly selected one, for two principal reasons. Intervals between societies were kept to as near three degrees of latitude or longitude as possible (excluding related cultures) in order to achieve a maximally even distribution over the earth's surface and thus the largest possible number of cases. Second, whenever an alternative presented itself, a hunting, gathering, fishing, or pastoral society was chosen in preference to an agricultural one to represent a particular local area. Throughout the world, agricultural societies preponderate so enormously over those with simpler economies that only through such a technique could a sample be made to include a sufficiently large and representative selection of nonagricultural cases.

The total sample includes 20 societies adjudged to subsist primarily by gathering, 30 by hunting, 21 by herding, 41 by fishing, and 210 by agriculture. The agricultural peoples were subdivided into 68 who derive their subsistence primarily from root or tree crops (nearly equivalent to horticulture), 64 who depend mainly on the extensive cultivation of cereal grains (roughly equivalent to swidden agriculture), and 78 who practise intensive agriculture, employing such advanced techniques as irrigation, fertilization, or the use of draft animals in ploughing. Each society was assigned to a particular typological category on the basis of the mode of subsistence estimated to provide the largest fraction of its food supply. In sixteen societies two modes of subsistence appeared to yield approximately equal amounts of food. These were classed as agricultural in the ten cases where agriculture was one of the two modes, as hunters in the five instances where hunting and gathering yielded approximately equal returns, and as a fishing society in the one other case of a tie (the Buduma of Lake Chad).

The data are presented in Table 1, which is preceded by an explanation of the symbols used. Analysis of the results follows.

Explanation of Table 1

Latitude. The individual societies are ordered by latitude, from south to north in Africa, the Circum-Mediterranean region, and the Insular Pacific; and from north to south in East Eurasia and North and South America.

Longitude. Societies of the same latitude are ordered within each region by longitude — from west to east. The ordering is designated to facilitate confirmation of the fact that no two societies listed are closer to one another than three degrees of either latitude or longitude, i.e., not closer than 200 miles apart.

Name of Society. Preference is given to the commonest usage.

Code Number. This number refers to the listing of the society in the "Ethnographic Atlas" (*Ethnology*, vols. 1–5, 1962–66), where the reader may find a precise identification of the society in space and time, together with alternative names, a selected bibliography, and other coded ethnographic data. A few societies have not as yet been listed in the *Atlas* but will shortly appear there; most of these are marked "(WES)," indicating that they are listed in the "World Ethnographic Sample" (Murdock 1957).

Subsistence (two columns). The symbols in one column indicate the basic subsistence technique, in the other column auxiliary techniques of importance.

G — gathering of plant foods;

H — hunting, trapping (land animals), and fowling;

F — fishing, shellfishing, and the pursuit of aquatic animals;

L — herding of large domesticated animals, e.g., camels, cattle, reindeer;

S — tending of small domesticated animals other than the dog, e.g., goats, pigs;

R — cultivation primarily of root or tree crops, i.e., essentially horticulture;

C — cultivation of cereal grains by extensive, e.g., swidden, techniques;

I — intensive agriculture, e.g., with use of irrigation, fertilization, or the plough.

The numbers indicate percentages of the total food supply produced by the particular technique; thus 5 means approximately 50 per cent, 2 means 20 per cent. Where two techniques are of about equal importance, agriculture, if it is one of them, is listed as basic and the other as auxiliary; in other ties, hunting or fishing is similarly given precedence.

Settlement Pattern. Four major categories are distinguished, as follows: (1) fully nomadic (nom.); (2) seminomadic (seminom.), i.e., when a migratory band occupies a fixed settlement for a season, e.g., winter quarters; (3) semisedentary (semised.), i.e., when a community shifts regularly from one to another fixed settlement or occupies permanently a single settlement from which a substantial portion of the population departs seasonally, e.g., during transhumance; (4) fully sedentary (sed.). The sedentary settlement pattern is to be understood as implying nucleated villages or towns except when the symbols 'N' or 'H' are added, referring respectively to neighbourhoods of dispersed homesteads and clusters of separated hamlets.

Average Local Group. The categories employed are as follows: (1) a mean population of fewer than 50 persons; (2) a mean population of 50 to 99; (3) a mean population of 100 to 199; (4) a mean population of 200 to 400; (5) a mean population of more than 400 in the absence of towns of 5,000 or more; (6) the occurrence of at least one indigenous town of more than 5,000 inhabitants in the absence of cities; (7) the occurrence of at least one indigenous city with more than 50,000 inhabitants. Data on mean community size are available for only 192 of the 322 societies listed.

Table 1—DATA ON 322 MAXIMALLY DISTRIBUTED SOCIETIES

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
37S	6W	Tristan da Cunha	Cf3:226	R4	F3, L2	Sed.	100–199
29S	31E	Zulu	Ab12:404	C5	L4	Sed. N	—
26S	18E	Hottentot	Aa3:102	L5	H3	Nom.	100–199
25S	46E	Antandroy	Eh7:613	L4	C3	Semised.	—
24S	27E	Tswana	Ab13:405	C5	L4	Sed.	Towns
24S	32E	Thonga	Ab4:104	C5	L3	Sed.	—
21S	16E	Herero	Ab1:2	L6	H3	Nom.	—
20S	21E	Kung Bushmen	Aa1:1	G8	H2	Nom.	< 50
19S	46E	Merina	Eh2:46	I7	L2	Sed.	Towns
16S	27E	Ila	Ac1:4	C6	L3	Semised.	200–400
15S	23E	Lozi	Ab3:103	I6	L3	Semised.	50–99
13S	36E	Yao	Ac7:304	C6	F2	Sed. H.	100–199
12S	16E	Mbundu	Ab5:203	C5	L2	Sed.	200–400
12S	22E	Luvale	Ac11:668	C6	F3	Sed.	—
11S	31E	Bemba	Ac3:105	C7	H1	Sed.	100–199
9S	34E	Nyakyusa	Ad6:208	I6	L2	Sed.	100–199
8S	26E	Luba	Ae6:210	R4	H2, F2	Sed.	—
8S	38E	Luguru	Ad14:704	C8	S1	Sed. H	—
5S	13E	Yombe	Ac8:305	C5	H2, F2	Sed.	200–400
5S	22E	Kuba	Ac4:106	C6	F2	Sed.	100–199
5S	32E	Nyamwezi	Ad20:761	C7	S1	Sed.	50–99
4S	26E	Songola	Ae11:670	F5	R2, H2	Sed.	< 50
3S	35E	Hatsa [now the preferred name]	Aa9:726	H5	G5	Nom.	< 50
2S	30E	Ruanda	Ae10:641	I5	L4	Sed. N	—
2S	41E	Bajun	Ad1:6	F5	C4	Sed.	—
0	24E	Nkundo	Ae4:110	R6	F2	Sed. H	—
0	36E	Dorobo	Aa2:101	H6	G4	Nom.	< 50

Table 1—DATA ON 322 MAXIMALLY DISTRIBUTED SOCIETIES—(Cont.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
1N	11E	Fang	Ae3:109	R5	H2	Sed.	50–99
2N	20E	Ngombe	Ae39:818	R6	H2, F2	Sed.	100–199
2N	28E	Mbuti Pygmies	Aa5:202	G6	H3	Nom.	< 50
3N	25E	Babwa	Ae7:308	C7	H2	Sed. H	—
3N	32E	Lugbara	Ai32:1047	C5	L4	Sed. H	—
4N	11E	Kpe	Ae2:9	R5	L3	Sed.	50–99
4N	35E	Turkana	Aj5:220	I4	L3, G2	Seminom.	—
5N	5W	Avikam	Af45:945	R4	F4	Sed.	—
5N	8E	Ibibio	Af20:920	R6	F2	Sed. H	> 400
7N	2W	Ashanti	Af3:111	R7	F2	Sed.	Towns
7N	22E	Banda	Ai1:15	C7	H2	Sed. N	—
7N	29E	Bongo	Ai35:1050	C5	G2	Sed. N	—
8N	11W	Mende	Af5:211	C7	S1	Sed.	> 400
8N	4E	Yoruba	Af6:212	R8	S1	Sed.	Cities
8N	10E	Jukun	Ah2:115	C6	S2	Sed.	—
8N	32E	Nuer	Aj3:120	L5	C4	Seminom.	—
8N	40E	Arusi	Ca11:841	L5	C4	Semised.	—
8N	48E	Somali	Ca2:19	L9	I1	Nom.	< 50
9N	18E	Sara	Ai22:1037	C6	L3	Sed. N	—
9N	35E	Mao	Ai46:1061	C6	L1	Sed.	—
10N	4W	Lobi	Ag11:674	C6	G2	Sed. N	100–199
10N	1E	Konkomba	Ag10:674	C6	L1	Sed. H	—
10N	14E	Mundang	Ag15:1030	C5	L4	Sed.	—
11N	16W	Bijogo	Ag18:963	C5	L2, F2	Sed.	—
11N	13W	Futajalonke	Ag6:214	C5	L5	Sed.	—
11N	8E	Zazzagawa	Cb26:1084	I6	L3	Sed.	Cities
11N	11E	Bolewa	Cb7:680	C6	L2	Sed.	—

Table 1—Data on 322 maximally distributed societies—(Cont.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
12N	24E	Fur	Cb17:875	I6	L3	Sed.	—
12N	31E	Tullishi	Ai42:1057	C6	L3	Sed. H	—
13N	5E	Bororo Fulani	Cb8:681	L8	C2	Semised.	50–99
13N	15E	Buduma	Cb5:321	F4	L4, C2	Sed.	—
13N	38E	Amhara	Ca7:679	I6	L3	Sed. H	Cities
14N	5W	Bozo	Ag7:312	F6	I3	Semised.	—
15N	17W	Wolof	Cb2:21	C6	L3	Sed.	100–199
15N	10E	Soninke	Ag25:870	C7	L2	Sed.	—
15N	45E	Yemeni	Cj9:915	I7	L3	Sed.	Towns
17N	1W	Songhai	Cb3:122	I5	L3, F2	Sed.	Towns
20N	35E	Bisharin	Ca5:320	L8	I2	Seminom.	—
21N	17E	Teda	Cc2:23	I5	L3, G2	Seminom.	—
22N	13W	Regeibat	Cc1:22	L7	I2	Seminom.	—
23N	6E	Ahaggaren	Cc9:880	L5	I3	Nom.	—
23N	33E	Barabra	Cd1:24	I6	L3	Sed.	50–99
28N	16W	Guanche	Cd11:897	C6	S2, F2	Sed.	—
29N	26E	Siwans	Cc3:123	I7	L3	Sed.	>400
30N	31E	Anc. Egyptians	Cd6:867	I7	L2	Sed.	Cities
31N	2E	Algerians	Cd12:899	I7	L3	Sed.	Cities
32N	35E	Anc. Hebrews	Cj3:230	I6	L3	Sed.	—
32N	44E	Kurd	Ci11:913	I5	L4	Semised.	—
35N	4W	Riffians	Cd3:125	I5	L3	Sed.	Towns
36N	4E	Kabyle	Cd4:224	I7	L3	Sed.	>400
38N	30E	Turks	Ci5:653	I5	L4	Sed.	Cities
39N	23E	Greeks	Ce7:708	I6	L3	Sed.	Cities
40N	42E	Armenians	Ci10:912	I7	L3	Sed.	Cities
42N	7W	Portuguese	Ce2:26	I5	L4	Sed.	Cities

Table 1—DATA ON 322 MAXIMALLY DISTRIBUTED SOCIETIES—(Cont.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
42N	12E	Imperial Romans	Ce3:126	I6	L2, F2	Sed.	Cities
42N	20E	Albanians	Ce1:25	I6	L4	Sed. N	—
42N	48E	Kumyk	Ci3:131	I7	L3	Sed.	—
43N	1W	Basques	Ce4:225	I5	L3, F2	Sed. N	Cities
43N	23E	Bulgarians	Ch5:347	I7	L3	Sed.	Cities
44N	42E	Cherkess	Ci4:229	I5	L4	Sed. H	—
47N	20E	Hungarians	(WES)	I7	L3	Sed.	Cities
49N	24E	Hutsul	Ch2:130	I4	L4	Sed. N	Towns
50N	5E	Walloons	Cg5:227	I6	L3	Sed.	Cities
53N	7E	Dutch	Cg1:28	I6	L3	Sed.	Cities
57N	46E	Cheremiss	Ch4:323	I5	L3, F2	Sed.	—
64N	20W	Early Icelanders	Cg2:29	L4	F2, H2	Semised.	—
68N	22E	Lapps	Cg4:129	L6	H2, F2	Nom.	< 50
72N	145E	Yukaghir	Ec6:236	H5	F4	Seminom.	< 50
68N	75E	Yurak Samoyed	Ec4:136	L4	H3, F3	Nom.	< 50
66N	97E	Khalka Mongols	Eb3:134	L8	H1	Seminom.	< 50
66N	177E	Reind. Chukchee	Ec3:135	L5	F3, H2	Nom.	< 50
65N	125E	Yakut	Ec2:38	L4	H2, F2	Seminom.	50–99
62N	74E	Ostyak	(WES)	F4	L3, H3	Seminom.	—
62N	90E	Ket	Ec8:360	F6	H3	Seminom.	100–199
62N	164E	Maritime Koryak	Ec5:235	F5	L3	Semised.	100–199
53N	142E	Gilyak	Ec1:37	F5	H3, G2	Seminom.	50–99
48N	70E	Kazak	Eb1:35	L8	H1	Seminom.	50–99
47N	132E	Goldi	(WES)	F5	H3, C2	Sed.	—
45N	126E	Manchu	Ed3:137	I7	L3	Sed.	—
44N	144E	Ainu	Ec7:325	F4	H3, G2	Sed.	—
39N	100E	Monguor	Eb2:36	I6	L3	Sed. H	—

Table 1—DATA ON 322 MAXIMALLY DISTRIBUTED SOCIETIES—(Cont.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
39N	118E	Shantung Chinese	(WES)	I7	L2	Sed.	Cities
38N	62E	Turkmen	Eb5:234	I6	L4	Sed.	—
38N	127E	Koreans	Ed1:39	I6	L2, F2	Sed.	Cities
37N	75E	Burusho	Ec2:139	I6	L4	Sed.	200–400
35N	68E	Afghans	(WES)	I5	L4	Sed.	> 400
35N	73E	Kohistani	Ea4:232	I4	L4	Semised.	—
35N	136E	Japanese	Ed5:287	I7	F2	Sed.	Cities
34N	63E	Moghul	Ea7:709	I6	L4	Sed. H	100–199
34N	75E	Kashmiri	Ef8:1092	I7	L2	Sed.	Cities
33N	48E	Bakhtiari	(WES)	L8	I1	Nom.	—
30N	54E	Basseri	Ea6:358	L8	H1	Nom.	> 400
30N	78E	Pahari	Ef7:612	I6	L4	Sed.	Cities
30N	91E	Tibetans	Ee4:239	I6	L4	Sed.	—
29N	103E	Lolo	Ed2:40	I6	L3	Sed.	100–199
28N	87E	Sherpa	Ee6:630	I5	L5	Semised.	200–400
27N	94E	Ao Naga	Ei14:422	C6	L2	Sed.	> 400
26N	91E	Garo	Ei1:47	C8	L1	Sed.	100–199
26N	97E	Kachin	Ei5:246	C6	L2	Sed.	100–199
26N	100E	Minchia	Ed8:361	I7	L2	Sed.	> 400
26N	107E	Magpie Miao	Ed4:136	I6	L2	Sed. H	—
26N	128E	Okinawans	Ed7:326	R6	L2, F2	Sed.	Cities
25N	68E	Sindhi	Ea1:34	I5	L3, F2	Sed.	Cities
24N	115E	Min Chinese	Ed6:326	I7	F2	Sed.	Cities
23N	88E	Bengali	Ef2:141	I6	L2, F2	Sed.	Cities
22N	74E	Bhil	Ef5:328	I4	L2, F2	Sed. N	50–99
22N	85E	Bhuiya	Eg7:329	C5	L2	Sed.	50–99
22N	93E	Lakher	Ei4:147	C5	H2	Sed.	—

Table 1—DATA ON 322 MAXIMALLY DISTRIBUTED SOCIETIES—(Cont.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
22N	96E	Burmese	Ei3:146	I6	L2, F2	Sed.	Cities
21N	80E	Maria Gond	Eg3:142	C6	S2, G2	Sed.	50–99
21N	105E	Muong	(WES)	I6	F2	Sed.	—
20N	101E	Lamet	Ej1:49	C5	L2, H2	Sed.	50–99
19N	109E	Li	Ed9:361	I7	L1	Sed.	100–199
18N	79E	Telugu	Eg10:688	I8	L3	Sed.	Cities
18N	82E	Bondo	(WES)	I7	L3	Sed.	—
17N	97E	Karen	Ei7:364	I6	F2	Sed.	—
17N	107E	Vietnamese	Ej4:149	I6	L2, F2	Sed.	Cities
16N	79E	Chenchu	Eg1:43	H8	L1	Semised.	< 50
15N	100E	Thai	Ej9:367	I7	F2	Sed.	Cities
13N	108E	Rhade	Ej10:456	C8	L1	Sed. H	—
12N	77E	Toda	Eg4:143	L9	G1	Semised.	< 50
12N	93E	Andamanese	Eh1:45	F4	G4, H2	Seminom.	< 50
12N	98E	Selung	Ej6:249	F8	H1	Nom.	—
12N	105E	Cambodians	Ej5:248	I6	L2, F2	Sed.	Cities
9N	92E	Nicobarese	Eh5:244	R5	F2	Sed. H	—
8N	81E	Vedda	Eh4:145	G4	H3, F3	Seminom.	< 50
5N	102E	Semang	Ej3:148	G4	H3, F3	Nom.	< 50
42S	146E	Tasmanians	(WES)	H4	F3, G3	Nom.	< 50
35S	175E	Maori	Ij2:167	R4	H2, F2, G2	Sed.	—
24S	134E	Aranda	Id1:56	G6	H4	Nom.	< 50
22S	165E	Ajie	Ih5:263	R6	F3	Sed.	—
21S	118E	Kariera	Id5:256	H5	G3, F2	Nom.	< 50
20S	174W	Tongans	(WES)	R5	F3	Sed.	—
20S	135W	Mangarevans	Ij7:658	F6	R4	Sed.	—
18S	179E	Lau Fijians	Ih4:165	R5	F4	Sed.	50–99

Table 1—DATA ON 322 MAXIMALLY DISTRIBUTED SOCIETIES—(Cont.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
16S	168E	Bunlap	Ih3:164	R7	S1	Sed.	50–99
14S	142E	Wikmunkan	Id6:333	H4	G4, F2	Nom.	< 50
13S	177E	Rotumans	Ih6:337	R7	F2	Sed.	—
12S	131E	Tiwi	Id3:157	G5	H3, F2	Nom.	100–199
11S	123E	Rotinese	Ic4:253	C7	L2	Sed.	100–199
11S	166E	Santa Cruz	(new)	R4	F3	Sed.	—
11S	166W	Pukapukans	Ii3:166	R5	F4	Sed.	100–199
10S	120E	E. Sumbanese	(WES)	I7	L2	Sed. N	—
10S	144E	Miriam	Ie14:657	R6	F3	Sed.	—
10S	161E	Ulawans	Ig6:262	R6	F3	Sed. H	100–199
9S	143E	Kiwai	Ie13:656	R5	H2, F2	Sed.	—
9S	147E	Motu	Ie10:631	R5	F4	Sed.	> 400
9S	154E	Rossel	Ig11:693	R3	F3, G3	Sed.	< 50
9S	140W	Marquesans	Ij3:168	R5	F4	Sed. N	50–99
8S	115E	Balinese	Ib3:152	I6	L3	Sed.	—
8S	125E	Alorese	Ic2:154	C7	G2	Sed.	100–199
8S	131E	Tanimbarese	Ic6:332	R5	F2	Sed.	—
8S	151E	Trobrianders	Ig2:62	R5	F3	Sed.	100–199
8S	178E	Ellice	Ij4:264	F5	R4	Sed.	—
7S	110E	Javanese	Ib2:54	I7	L2	Sed.	Cities
6S	145E	Siane	Ie17:713	R7	S1	Sed.	200–400
6S	155E	Siuai	Ig1:61	R5	S2, G2	Sed. H.	—
5S	119E	Macassarese	Ic1:55	I6	L2, F2	Sed. N	100–199
5S	160E	Ontong-Java	Ii5:265	R5	F5	Semised.	—
4S	136E	Kapauku	Ie1:57	R6	S2	Sed.	50–99
4S	142E	Kwoma	Ie12:655	R4	G3, H2	Sed. H	—
3S	100E	Mentaweians	Ib7:369	R6	F2	Sed.	—

Table 1—DATA ON 322 MAXIMALLY DISTRIBUTED SOCIETIES—(Cont.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
3S	103E	Kubu	(WES)	H3	G3, R2, F2	Nom.	—
3S	153E	Lesu	Ig4:163	R4	F4	Sed. H	200–400
2S	121E	Toradja	Ic5:254	C6	L2, H2	Sed.	—
2S	143E	Aua	(WES)	F5	R4	Sed.	—
2S	147E	Manua	Ig9:387	F9	S1	Sed.	100–199
1S	135E	Nauruans	If13:432	R6	F4	Sed. N	200–400
1N	128E	Tobelorese	(WES)	C5	F2, G2	Sed.	—
2N	99E	Batak	Ib4:153	I7	L3	Sed.	—
2N	112E	Iban	Ib1:53	C6	S2, F2	Sed.	—
3N	173E	Makin	If14:633	R4	F4	Sed.	200–400
6N	117E	Dusun	Ib5:251	I5	L2, F2	Sed.	200–400
7N	135E	Palauans	If1:59	R5	F4	Sed.	—
7N	143E	Woleians	If4:161	R5	F4	Sed.	50–99
7N	152E	Trukese	If2:60	R5	F5	Sed.	100–199
7N	158E	Ponapeans	If5:260	R6	F3	Sed. N	< 50
8N	123E	Subanun	Ia4:151	C9	S1	Sed. N	< 50
8N	126E	Manobo	(WES)	C7	H1	Sed. N	—
9N	128E	Yapese	If6:260	R5	F4	Sed. N	—
12N	165E	Bikinians	If12:431	R5	F4	Sed.	100–199
13N	121E	Hanunoo	Ia5:250	C7	L1	Sed. H	100–199
15N	145E	Chamorro	If8:427	C7	L2	Sed.	> 400
17N	121E	Ifugao	Ia3:150	I6	H2	Sed. H	100–199
20N	156E	Hawaiians	Ij6:376	R5	F4	Sed.	—
24N	121E	Atayal	Ia1:51	C6	S1	Sed.	100–199
68N	152W	Nunamiut	Na12:461	H7	F2	Seminom.	50–99
66N	135W	Kutchin	Na20:469	H5	F4	Seminom.	—
66N	37W	Angmagsalik	Na24:487	F8	H2	Semised.	< 50

Table I—Data on 322 maximally distributed societies—(Cont.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
63N	170W	Sivokakmeit	Na11:460	F8	H2	Sed.	100–199
63N	141W	Nabesna	Na1:68	H6	F2, G2	Seminom.	< 50
62N	160W	Ingalik	Na8:377	F5	H4	Seminom.	—
61N	145W	Eyak	Nb5:270	F5	H3, G2	Sed.	< 50
61N	120W	Slave	Na17:466	H5	F4	Nom.	50–99
59N	128W	Kaska	Na4:170	F5	H4	Seminom.	50–99
58N	70W	Naskapi	Na5:268	H7	F2	Seminom.	< 50
54N	167W	Aleut	Na9:458	F6	H3	Sed.	100–199
54N	132W	Haida	Nb1:70	F6	H2, G2	Sed.	—
52N	127W	Bellacoola	Nb9:471	F6	H2, G2	Sed.	50–99
52N	122W	Chilcotin	Nd8:550	F5	H3, G2	Seminom.	< 50
52N	98W	N. Saulteaux	Na33:496	H4	F4, G2	Seminom.	50–99
51N	112W	Blackfoot	Ne12:619	H8	G2	Nom.	200–400
50N	117W	Kutenai	Nd7:380	G4	H3, F3	Seminom.	—
48N	123W	Twana	Nb2:71	F6	H3	Seminom.	50–99
48N	106W	Assiniboin	Ne11:618	H7	G2	Nom.	200–400
47N	101W	Mandan	Ne6:341	I5	H2, F2	Semised.	—
47N	65W	Micmac	Na41:504	H5	F4	Seminom.	< 50
46N	88W	Menomini	Nf9:661	G4	C2, F2, H2	Semised.	—
45N	121W	Tenino	Nd1:73	F5	H2, G2	Semised.	200–400
45N	115W	Nez Perce	Nd20:562	F4	H3, G3	Seminom.	100–199
45N	108W	Crow	Ne4:178	H7	G2	Seminom.	—
44N	124W	Alsea	Nb28:511	F7	H2	Sed.	—
44N	85W	Potawatomi	Na42:66	H5	C3	Semised.	—
44N	78W	Huron	Ng1:79	C5	H4	Semised.	> 400
42N	120W	Kidutokado	Nd24:566	H4	G4, F2	Seminom.	< 50
42N	100W	Pawnee	Ng6:342	C5	H3	Semised.	200–400

Table 1—DATA ON 322 MAXIMALLY DISTRIBUTED SOCIETIES—(Cont.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
41N	124W	Yurok	Nb4:172	F5	G4	Sed.	<50
41N	96W	Omaha	Nf3:179	H4	I3	Semised.	50–99
41N	75W	Delaware	Ng6:279	C4	H2,F2,G2	Semised.	200–400
40N	112W	Uintah Ute	Nd58:610	H4	F3, G3	Seminom.	—
40N	86W	Miami	Nf4:276	C4	H3, G2	Semised.	>400
38N	120W	Miwok	Nc4:273	G6	H3	Semised.	—
37N	106W	Taos	Nh6:281	I7	H2	Sed.	>400
36N	115W	Moapa	Nd51:593	G6	H4	Seminom.	<50
36N	111W	Hopi	Nh18:442	I7	S1	Sed.	200–400
35N	118W	Kawaiisu	Nc27:542	G5	H3, F2	Seminom.	—
33N	113W	Maricopa	Nh5:280	G4	I3, F2	Sed.	50–99
33N	100W	Comanche	Ne3:177	H9	G1	Nom.	—
33N	84W	Creek	Ng3:180	C4	H2,F2,G2	Sed.	>400
32N	116W	Diegueno	Nc6:339	G6	H4	Seminom.	—
32N	91W	Natchez	Ng7:385	C5	H3, F2	Sed.	200–400
31N	108W	Chiricahua	Nh1:81	G6	H4	Nom.	—
29N	112W	Seri	Ni4:283	F6	H2, G2	Seminom.	—
29N	96W	Karankawa	Ne16:623	F4	H3, G3	Nom.	—
28N	107W	Tarahumara	Ni1:83	I5	L3	Sed. N	—
27N	82W	Calusa	(new)	F6	H2, G2	Sed.	>400
23N	104W	Huichol	Ni3:282	C5	H2	Sed. H	>400
22N	98W	Huastec	(WES)	C7	H2	Sed.	—
19N	99W	Aztec	Nj2:185	I7	F2	Sed.	Cities
19N	75W	Taino	(WES)	R6	F3	Sed.	—
18N	90W	Maya	Sa6:345	C7	F2	Sed.	—
16N	95W	Huave	Nj6:1105	F5	C4	Sed.	—
15N	61W	Callinago	Sb1:87	F5	R4	Sed. H	—

Table 1—DATA ON 322 MAXIMALLY DISTRIBUTED SOCIETIES—(Cont.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
14N	91W	Cakchiquel	(WES)	C8	H1	Sed. N	—
14N	88W	Lenca	(WES)	C7	H1	Sed.	—
13N	85W	Miskito	Sa9:390	G3	R2, H2, F2	Sed.	100–199
12N	72W	Goajiro	Sb6:391	L7	H1	Nom.	< 50
9N	83W	Talamanca	Sa5:287	C5	F2	Sed. N	—
9N	78W	Cuna	Sal:85	C6	F3	Sed.	50–909
9N	72W	Motilon	Sb3:188	G4	R3, F2	Sed.	< 50
9N	62W	Warrau	Sc1:88	G4	H3, F2	Semised.	50–99
6N	63W	Camaracoto	Sc11:450	R6	H2, F2	Sed.	—
6N	57W	Locono	Sc10:449	R5	F3	Sed.	< 50
5N	72W	Chibcha	Sf6:395	R8	H1	Sed.	Towns
5N	68W	Guahibo	Sc4:290	H4	F3, G3	Nom.	< 50
4N	76W	Paez	Sf5:348	R6	L2	Sed. N	—
3N	65W	Yekuana	Sc16:455	R3	H3, F2, G2	Sed.	—
3N	55W	Rukuyen	Sc14:453	R5	H2, F2	Sed.	—
3N	52W	Palikur	Sd3:292	R5	F3, H2	Sed.	—
1N	79W	Cayapa	Sf3:194	R5	F2	Sed. N	> 400
1N	71W	Tucano	Se5:293	R5	F3	Sed.	—
1N	59W	Waiwai	Sd7:722	R5	H3, F3	Sed.	< 50
1S	74W	Witoto	Se6:347	R4	H2, F2, G2	Sed.	—
3S	70W	Tucuna	Se2:92	R4	F4	Sed.	100–199
3S	46W	Tenetehara	Sj6:300	R5	H2	Sed.	—
4S	78W	Jivaro	Se3:191	R6	H2	Sed. N	< 50
4S	63W	Shiriana	Sd6:721	H4	F3, G3	Seminom.	50–99
5S	75W	Cocama	(WES)	F5	C4	Sed.	—
6S	58W	Mundurucu	Sd1:90	R4	H3, F2	Sed.	50–99
7S	45W	Timbira	Sj4:200	R4	H3, G2	Sed.	100–199

Table 1—DATA ON 322 MAXIMALLY DISTRIBUTED SOCIETIES—(Conc.)

Latitude	Longitude	Name of society	Code number	Basic subsistence	Auxiliary subsistence	Settlement pattern	Average local group
8S	52W	Coroa	Sj9:725	R6	H2	Seminom.	100–199
8S	35W	Tupinamba	Sj8:400	R4	H2,F2,G2	Sed.	200–400
9S	74W	Conibo	Se9:666	F4	R3, H2	Sed.	< 50
9S	48W	Sherente	Sj2:100	R4	H3, G2	Sed.	—
11S	52W	Tapirape	Sd2:190	R5	F3	Sed.	100–199
12S	59W	Nambicuara	Si4:199	G4	H3, R2	Seminom.	< 50
12S	53W	Trumai	Si2:98	R4	F3, G2	Sed.	< 50
12S	50W	Caraja	Sj1:99	F4	R2,H2,G2	Sed.	—
13S	72W	Inca	Sf1:93	I7	L2	Sed.	Cities
15S	58W	Paressi	(WES)	C6	H2, G2	Sed.	—
16S	69W	Aymara	Sf2:193	R6	L3	Sed.	> 400
16S	64W	Siriono	Se1:91	H5	G3	Seminom.	< 50
16S	55W	Bororo	Si1:97	H5	G4	Seminom.	—
18S	42W	Botocudo	Sj5:299	G5	H4	Nom.	50–99
19S	58W	Guato	Si6:350	G4	F3, H2	Seminom.	—
20S	63W	Chiriguano	Sh7:399	C5	F2	Sed.	—
20S	59W	Chamacoco	Sh6:398	G6	H4	Nom.	—
24S	63W	Mataco	Sh1:95	F4	H2, G2	Seminom.	50–99
25S	60W	Toba	Sh8:724	R3	H3, G2	Semised.	200–400
28S	50W	Aweikoma	Sj3:199	H6	G4	Nom.	< 50
29S	61W	Abipon	Sh3:196	H6	G2	Nom.	—
39S	68W	Araucanians	Sg2:195	I6	L2	Sed. H	50–99
46S	70W	Tehuelche	Sg4:349	H6	G2	Nom.	50–99
52S	74W	Alacaluf	Sg5:396	F7	H2	Nom.	< 50
55S	69W	Yahgan	Sg1:94	F7	H2	Nom.	< 50

For comparison with the prevailing mode of subsistence, the settlement pattern of each society was categorized as either nomadic, seminomadic, semisedentary, or sedentary (*see* Explanation of Table 1 for precise definitions). The correlations between subsistence norms and patterns of settlement are shown in Table 2.

Table 2 — Subsistence economy and settlement pattern

Type of economy	Nomadic	Semi-nomadic	Semi-sedentary	Sedentary	Totals
Gathering	8	6	3	3	20
Hunting	14	13	3	0	30
Pastoral	10	6	5	0	21
Fishing	4	11	4	22	41
Horticulture	0	1	2	65	68
Extensive cereal cultivation	0	0	5	59	64
Intensive agriculture	0	2	5	71	78
Totals	36	39	27	220	322

It is clear from Table 2 that societies with gathering, hunting, and pastoral economies tend to be nomadic or seminomadic. Among them there are only three societies that are classified as sedentary, and these exceptions are readily explained. All three (the Maricopa, Miskito, and Motilon) derive more of their food supply from gathering than from any other single subsistence activity, but all of them practise agriculture, and all three obtain more of their food from agriculture and fishing combined than they do from gathering.

It is equally clear that societies with agricultural economies of whatever type tend to be sedentary, or occasionally semisedentary. Again there are only three exceptions. The horticultural Coroa are reported to be seminomadic, but this is possibly an error since the other three Ge tribes in the sample are sedentary. The other two seminomadic agricultural groups are the Teda and Turkana, and it is noteworthy that both of them depend at least as strongly on a combination of herding, hunting, and gathering as they do on agriculture.

Fishing economies occupy an intermediate position. Indeed, an actual majority of the fishing societies in the sample are fully sedentary, and a number of the nomadic cases are readily explainable. The Selung or Sea Gypsies, for example, have no land settlements but live on their boats and wander at will. Fishing (including shellfishing and the pursuit of aquatic animals) is the only relatively simple mode of subsistence that appears conducive to a settled way of life, and it is highly probable that prior to the first appearance of agriculture about 10,000 years ago the only sedentary populations for many millennia were groups of fishermen. For hunters and gatherers to settle down to a settled life of agriculture a revolutionary readjustment of cultural habits is needed, but for fishermen the transition is vastly easier, and it is by no means improbable that fishing may have played a very important cultural–historical role in mediating the transition to early agriculture.

A widely accepted classification of subsistence economies is one in which the major divisions are food-collecting and food-producing. Our findings shed serious doubt on this logical dichotomy. So far as settlement patterns are concerned, at least the food-producing technique of animal husbandry has effects resembling those of the food-collecting techniques of hunting and gathering, whereas food-collecting by fishing techniques in its consequences resembles food-production by agriculture. Not only are fishing societies commonly sedentary, but they often support social institutions of much greater complexity than are found among hunters or gatherers. One thinks, for example, of the technological and artistic virtuosity of the Eskimo and the Northwest Coast peoples, of the intense elaboration of trade in Manus and other fishing societies of Oceania and among the Tenino and other Plateau tribes around The Dalles on the Columbia River. One also thinks of the stratified class system and complex political organization of the Calusa of South Florida (cf., Goggin and Sturtevant 1964). It is almost equally inappropriate to rank other fishing societies such as the Bajun of Somalia, the Bozo of the Niger, the Callinago of the Lesser Antilles, and the Cocama of the Amazon in the same category with nomadic hunters and gatherers.

The vast majority of sedentary peoples were found to occupy nucleated villages or other forms of settlement with a compact core. Only twenty-four societies of the sample live in neighbourhoods of dispersed family homesteads, and a like number in clusters of separated hamlets. These alternative physical arrangements reveal no trends toward association with particular modes of subsistence, from which it may be concluded that the causal factors lie elsewhere, possibly in the patterns of land tenure or in the relative frequency of warfare.

Table 3 — SUBSISTENCE ECONOMY AND SIZE OF LOCAL COMMUNITY
(in percentages)

Type of economy	Mean of				>400 no towns	Towns of 5,000	Cities of 50,000	Number of cases
	<50	50–99	100–199	200–400				
Gathering.	61	23	16	0	0	0	0	13
Hunting.	61	29	0	10	0	0	0	21
Pastoral.	59	25	8	0	8	0	0	12
Fishing.	40	23	27	5	5	0	0	22
Horticulture. . .	18	18	28	16	10	5	5	39
Extensive cereal cultivation. . .	3	16	36	19	23	3	0	31
Intensive agriculture. . .	0	7.5	11	7.5	9	9	56	54

Data were also compiled on the average population size of local communities (bands, villages, etc.) for the 192 societies of the sample for which such information was available. The results, which are presented in Table 3 in percentages rather than actual numbers for the sake of clarity, reveal some striking correlations with subsistence norms. The categories, which are defined in detail in the explanation of Table 1, range from a mean local population of less than 50 persons at the bottom of the scale to the presence of at least one indigenous city with more than 50,000 inhabitants at the top.

Reference to Table 3 reveals that local communities tend to be very small in gathering, hunting, pastoral, and fishing societies alike. In only five of the 68 societies in these categories for which data are available do local groups attain an average population of 200 or more. These are the Assiniboin and Blackfoot of the Plains, who hunt buffalo with horses; the Basseri of Iran, transhumant pastoralists; and two fishing tribes, the Calusa and the Tenino. Horticulturalists and extensive cereal cultivators occupy, on the average, appreciably larger settlements. Eight of the 70 societies with economies of these types have indigenous towns of 5,000 or more, and two groups which emphasize root crops, the Okinawans and Yoruba, even have indigenous cities.

The most striking fact that emerges from Table 3, however, is that only 18.5 per cent of the societies that practise intensive agriculture live in settlements averaging fewer than 200 inhabitants, whereas 56 per cent, a substantial majority of the total, actually have cities larger than 50,000. This suggests the conclusion that the so-called Urban Revolution in human culture history had as its prerequisite condition the achievement, not so much of agriculture as such, but of intensive agricultural techniques like irrigation, fertilization, and the plough. Once these techniques had been acquired, moreover, it would seem that the development of cities became, not merely possible, but probable and perhaps even inevitable.

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DISCUSSION

Agriculture and Cities

LEE (to Murdock): Is it possible to reverse the argument that intensive agriculture leads to the growth of the cities? As suggested by Netting's paper in this volume, the growth of population creates the necessity of the intensification of food production. In other words the causal flow may run in the opposite direction from that conventionally conceived.

MURDOCK: In studies of this kind one gets no indication of causal flow. All that is seen is correlation.

LEE: Nevertheless, in understanding the origins of the situation the suggestion of causality seems implicit.

MURDOCK: I am basically suggesting that until there is intensive agriculture there are no cities. There are urban aggregates smaller than cities, however, without intensive agriculture.

HELM: Braidwood now appears to regard some of the "hilly flank" settlements of the Near East as permanent mesolithic towns. His evidence suggests that for a period of a few thousand years there was in that region sedentary occupation based on a food-gathering economy with no evidence of the development of horticulture.

MURDOCK: But those settlements were all located on streams. Fishing or aquatic mammal hunting may have been emphasized. For instance, prehistoric data on the Nile valley shows how important hippopotami were in the pre-agriculture period. If we substitute fishing for hunting aquatic mammals, we could easily see how a group could take on agriculture without a change in settlement pattern.

Seminomadism versus Semisedentariness

HELM (to Murdock): Could you elaborate on the distinction between *seminomadic* and *semisedentary* types?

MURDOCK: I have made a rather sharp distinction between the two. *Seminomadic* groups are those which are normally migratory but settle in winter quarters for a few months each year — they aggregate for a few months. The distinction between them and the strictly nomadic people, such as the Bushmen or the Hadsa, is primarily climatic. *Semisedentary* refers to two types of settlement pattern. The first type would be represented by the Toda, who rotate during the year among the settlements in various parts of their territory. Another example of this type is the Tenino, who leave their fishing camps on the Columbia or on its tributaries and retire for the winter to another permanent village for about four months; thus rotating between two permanent settlements.

The second type of semisedentary settlement pattern is represented by transhumant pastoralists who also practise agriculture. The Berbers of the mountains of Morocco have permanent fortified villages, which are occupied throughout the year by part of the population; however, a substantial part of the population leaves for the bulk of the year in transhumant marches related to pastoralism. The distinction here is much sharper than that existing between *seminomadic* and *nomadic*.

NESBITT: For *semisedentary* the climatic factor would not hold true as it does in the case of *seminomadic*.

MURDOCK: Yes, but geographical factors would.

DAMAS: With reference to Dr. Murdock's charts, most sorts of economies seem to form a continuum from left to right, whereas with regard to fishing, there seems to be a bimodal distribution. There are a number of *nomadic* and a number of *sedentary* groups but very few *semisedentary* examples. Is there any factor that might account for this?

MURDOCK: We might look at the proportion of subsistence provided by fishing. Among the Northern Athapaskans, fishing is the single most productive source of food, but it is combined with hunting. The Athapaskans fish at a number of places. That kind of situation encourages nomadism. The Calusa of south Florida had large villages sometimes estimated at 10,000 inhabitants each, and certainly they were in excess of 1,000 each. They were very productive shell fishers and were strictly sedentary. The foregoing types might represent examples of the bimodal pattern.

DAMAS: It appears that the *semisedentary* category is represented quite evenly by all the basic subsistence types in the sample. Perhaps it represents a kind of transitional phase.

Units of Survey

DAMAS (to Murdock): With regard to your figures used for community size, in band societies there are usually minimal and maximal aggregations during the course of the year. Helm's "regional" and "local" bands appear to correspond to these. How did you determine the unit used in your survey?

MURDOCK: Actually I was strongly influenced by Helm's distinctions, though much depends upon whether the regional band actually aggregated often enough to comprise a group with a great deal of face-to-face interaction.

DAMAS: I think Helm will agree that the regional band concept is used in two ways: (1) as applied to entities that actually aggregated at some time during the year's cycle and (2) those which are more or less merely conceptual units.

MURDOCK: The problem is more difficult in the case of hunting–gathering and pastoral peoples as opposed to sedentary agricultural populations. I arrived at the chart figures in the following manner: in most cases there are no statistics available listing the number of villages and their populations, but often there are approximate estimates. Or, if the source gives the total population of the tribe and then lists the villages, I simply divide the total population by the number of villages to get the average village population. Unfortunately in most demographic aspects of anthropology our information is woefully and discouragingly weak.

Duration of Occupation

LEE: Could I suggest a distinction between *group specific settlement pattern* and *locality specific settlement pattern*? A group may move six times a year, but the lifetime of one occupation site may extend over a period of years. There is a basic difference involved depending on whether we mean people moving or camps moving.

MURDOCK: Occupation of sites might vary at different times of the year, a short period at one season and a longer one at another. The puzzle that the archaeologists then face is determining whether different sites are occupied by the same people under such circumstances.

HELM: This relates to the problem of defining true nomadism. In band societies I think there are, almost always, stations that are not only occupied in succession by a group but also may be inhabited by different people seasonally.

BICCHIERI: It is for this reason that in my paper I called this phenomenon *cyclic migration* rather than *nomadism*.

LEE: If we take the question of duration of occupancy of each site and try to develop a system of quantifying sedentariness versus nomadism, we will get remarkable disparities which will correlate with types of subsistence. Might it not be useful to consider the societies studied by members of the panel and compare the durations of occupancy?

(To Damas): For instance, how many sites might be occupied during the year by the Central Eskimo?

DAMAS: In the three regions with which I am familiar, during the winter-sealing period, the camps on the sea-ice would usually relocate every month. During the summer over a five- or six-month period, there might be an average of one move per month. I would guess that approximately five to fifteen sites might be occupied in one year.

LEE: We could regard the Central Eskimo then as having a duration of habitation of 1/12. In other words, the life of the site is one month. Using the same scale I would put the Bushmen at 3/12.

(To Bicchieri): What would be the case for the BaMbuti?

BICCHIERI: I would say a maximum of 1/12.

LEE: Let us consider other examples.

NETTING: With the Kofyar, their duration of settlement is thirty years or more. Homesteads are continuously occupied for generations.

RAPPAPORT: The Maring, I would say, remain at a place between one and fifteen years.

MURDOCK: The Tenino averaged about one hundred years at a site.

LEE: At this point we should introduce the other variable which is reoccupation of a single site. If a group lives for two months of each year at a given habitation (2/12) and returns for twenty years, that is not to be coded as twenty years of occupation. Rather, the total lifetime of occupancy would be only forty months.

The scheme as it is worked out with these few cases shows a wide disparity between hunters and gatherers, on the one hand, as opposed to agriculturalists or fishing people, on the other. With more examples we could develop a continuum from nomadism to sedentarism. We could have a hard, quantifiable index from 1/12 (Mbuti), to 1–15 (Maring), to 30+ (Kofyar).

This index relates to other aspects of the culture. I want to suggest that there is an investment in land that influences how elaborate the house structures that are erected there will be. There is an energy investment — how much labour goes into a piece of land; and a symbolical investment — how much this land means to a people. With low investment it is relatively easy to move; with high investment it is more difficult to move.

NESBITT: While these ethnographic examples comprise a wide range, if we refer to archaeology there are more extreme examples. For example, paleolithic people resided in caves for 200 to 600 years. In North Africa the people who relied on fresh-water snails unquestionably inhabited permanent sites for 600 to 1,500 years.

LEE: The archaeologists cannot in many cases determine permanent or intermittent occupation from the deposit, but if we can provide the ethnographic data on seasonal settlement patterns we should be able to help them in their interpretations.

RELATIONSHIP BETWEEN SETTLEMENT PATTERN AND COMMUNITY PATTERN

BY

JUNE HELM

Introductory Remarks

I find useful Chang's conceptual distinction between the *settlement pattern* of a society, the physical occupations of locale in space, and the *community pattern*, the social group aspects of the people occupying locale. A society's complex of human aggregates in their time–space dimension is its settlement pattern. The relationships and arrangements, based in cultural convention, among occupants of locales are community pattern. These latter characterizations are mine. I may have expanded Chang's concepts for present purposes (his were archaeological), but I believe we are congruent in that we are both interested in distinguishing phenomena that can be "assumed or demonstrated to be related to cultural ecological forces from phenomena that can be attributed to efficient causes in the sphere of sociology and social psychology" (Chang 1962: 28). Generally, the archaeologist has only evidence of the settlement pattern, and only partial evidence at that, to work from.

I further suggest the addition to our formulation of what we may call *exploitative pattern*. I have elsewhere (Helm 1969) defined exploitative pattern to be "the total set of activities in the acquisition of life's goods through the application of technology upon environment."

Let me explore briefly the interplay of these patterns. It is essential to any society that it command an exploitative pattern efficient enough to maintain a biologically viable population in space and through time. Exploitative patern — resulting from cultural definition of environmental resources and of the tools and techniques for the utilization of those resources — imposes broad limits and requirements upon settlement pattern. Bushmen cannot maintain a permanent village of 5,000, given their hunting–gathering exploitative pattern. Settlement pattern comprises the society's forms of human occupations (for example, nucleated versus dispersed groups in their temporal, spatial, and size dimensions) existing through seasonal or other exploitative cycles. Settlement pattern would seem to set boundaries of possibility for community pattern. For example, the well-developed chiefdom evidently cannot exist under seminomadic settlement conditions predicated on a gathering economy. However, we all are aware that cultural definitions of proper social relations and also of human pleasures and stresses can be efficient explanations of size, duration, and arrangement of aggregates. We have a notable example in Bicchieri's observation of the social–psychological uses to which the honey season is put by the BaMbuti. Here, following Professor Bicchieri's argument, we see an aspect of the BaMbuti settlement pattern (seasonal nucleation versus dispersal) shaped by social sentiments ("Whom do I wish to be with, or get away from, given the opportunity of this season?").

We cannot predicate a simple progression of exploitative pattern-shaping settlement pattern which in turn shapes community pattern. In the long run of the human condition there may be flow in this direction, but in any short run there is certainly interdependence and feedback. Ideological and social directives can and do affect settlement and exploitation.

I offer this schema only as a possibility — that it may help us to segregate components of the complex whole which somehow has to do with cultural ecology in order to comprehend the total domain more clearly, possibly even eventually to determine some order of preemptive variables.

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DISCUSSION

Definitions

Murdock (to Helm): What you call 'community pattern' is our conception of 'social organization,' which is really concerned with norms and rules, etc. Now, if the problem arises of an increasing or decreasing population over time, do you take that as a factor of community or of settlement pattern?

Helm: To me, the number of people is a determining factor of settlement form. The social ties between the persons, in so far as the number is determined by cultural directives (e.g., the corroboree gathering), I see as an aspect of the community pattern.

Murdock: Perhaps the settlement pattern is some sort of compromise between exploitative and community patterns and is influenced by both and expressed on the surface of the land in the way that groups actually aggregate in space.

Helm: If I wanted to diagram the situation as I see it, I would draw arrows from both exploitative pattern and community pattern toward settlement pattern: the latter appears to be more of a dependent variable. But even the exploitative pattern involves the placing of persons often in accord with social directives, as for instance in the case of a pair of brothers-in-law operating as trapping partners, a common arrangement among the Déné. None of the three are discrete categories, though they are still useful for comparative purposes. The settlement pattern is especially useful for comparison because it is the most measurable of the three. That is, it is easier to compare concrete data on settlement than to try to objectify what is meant by a virilocal tendency or to attempt the comparison of the intricacies of two technologies. The settlement pattern may well provide the best jumping off point in inquiry, especially cross-societal inquiry.

Archaeological Contributions

Murdock: Does the archaeologist ever get direct evidence for community?

Winters: The evidence is much more inferred than direct. I have tried to show how a particular archaeological society was endogamous. In the realm of community or social organization it is a matter of inference or probability, but it is far more than mere restriction to the settlement itself.

I have been working on material from the Indian Knoll culture in Kentucky, specifically on their trade systems. The findings tell us something about the distribution of wealth in the society, an important feature of community organization. The population data form a normal curve when graphed. However, the graphs of the distribution of imported shells, an indicator of wealth, do not result in a similar curve. Marine shells show cyclical variation corresponding roughly to a two generational rise and fall; there were periods of intensive accumulation and periods of distribution of the wealth. At first the wealth was distributed over the whole range of the population. Later as wealth increased it was confined more and more to the hands of a smaller segment.

Murdock: It can be granted that the archaeologist gets direct evidence of exploitative techniques, the shells, the bones, the flora that were being used, etc. He can uncover village sites and can actually count the number of habitations. On the other hand, material on community organization is largely inferential. The ethnologist must contribute all he can regarding the relationships between community organization and (a) settlement and (b) exploitative techniques. The more conclusively such associations can be demonstrated by the ethnologist, the sounder the inferences the archaeologist can make.

Archaeologists have made substantial contributions regarding interpretation of descent or residence patterns. For example, Martin, using the suggestion (that I gave in *Social Structure*) that there had been a shift from the nuclear to the extended family in the Mogollon culture, noted the number of metates in house sites. Over time he found a decline in their number and hypothesized that in the earlier period every woman had her own metate, whereas later in the extended family situation, the women took turns with the family metate. However, the determination of lineage organization from the archaeological record is much more remote and must be preceded by much work by social anthropologists who have pinned down, or statistically validated, generalizations about types of dwelling, settlement pattern, and correlations with descent.

WINTERS: I agree that it is very difficult to isolate features of kinship organization with our present techniques and our present models. We can, however, deal very well with residence and similar data.

MURDOCK: If the archaeologist finds a site with dwellings clustered facing each other, he does not know whether these are polygamous or small extended families, but he has a sound basis for assuming that they are not independent nuclear families.

CARSTENS: In parts of South Africa, house sites can sometimes be seen from the air. For example, in Hottentot territory the stones that held the hut poles in position are often visible, as are the remains of the cooking huts. The patterns formed by these remains tell me a great deal. The outlines of three dwelling huts in a row with the outlines of cooking huts opposite the first and the third dwelling huts would suggest the strong possibility that this domestic cluster used to consist of: (a) a widower (or widow) living in the centre hut but without a kitchen, and (b) one nuclear family on each side with a kitchen. The kin group, moreover, would have been of the patrilineal extended family variety, and the widower (or widow) would have eaten with the nuclear family of one of his (or her) sons.

MURDOCK: The archaeologist can make sound inferences on the basis of such structures, but he cannot tell whether the residence pattern was patrilocal, avunculocal, or ambilocal, or whether clans existed in the society. Perhaps he can determine the existence of local exogamy and local endogamy.

Locality, Lineality, and Settlement Type

DAMAS: Are we in agreement that there is much electiveness in community patterns associated with specific settlement patterns? Using ethnological evidence, can we make additional statements regarding this relationship?

HELM: I think that we must first isolate factors of immediate relevance, as we have begun to do. I suggest that residence pattern is more immediately pertinent than is the kinship 'system' *per se*.

An interesting fact about residence is that, except for the Australians (and even there when one considers actual localized groups), the linkages tend to operate through primary relatives, either husband or wife, although there may be separate trends. In Damas's Eskimo material there are more men in the same camp in association with their brothers than with their sister's husbands, but overall there are linkages which tie a man to either his primary consanguines or his wife's primary consanguines.

DAMAS (to Helm): Did we not agree that in hunting societies, at least, a number of residence alternatives are available? Can we relate these variations to anything specific? I recall that even though we dealt in last year's conference with highly similar settlement patterns in the Eskimo and Déné areas, in your Déné examples there was a greater balance between male and female connections.

HELM: Your Iglulingmiut Eskimo made explicit statements about their preferences for proximity with brothers and fathers, and in actuality these preferences were evident in their associations. I did not get this sort of ideal expressed by the Déné, and the male versus female connections are indeed more balanced. These variant tendencies in residence decisions seem to relate to ideological differences.

NETTING: Going beyond mere residence alignments I think that it is very useful to study groups, such as work groups, that are mobilized from such alignments. We can ask for instance, "what is the optimum group size needed for farming a particular piece of land?" There are various ways that a group of seven men can be assembled to form such a party. Organization of such groups can be accomplished by activating various kinship features, but I am not sure that one way is any better than another. Variations occur in the way that such organizations are formed, and I would agree that in matters of descent there is often a wide range of alternatives available to be exploited with equal utility, without implying that the work group has to be much different in size or composition, whatever alternative is chosen.

MURDOCK: In most areas in the world when work groups and clustered households exist, they usually are formed of close consanguineal and affinal connections. People do not cluster in households without regard to kinship. They do not form canoe crews or work parties without reckoning with kinship. This being the case, the archaeologist who finds clusterings of various kinds can assume with a high degree of probability that the inhabitants were somehow related, probably closely, either consanguinally or affinally.

DAMAS: Earlier in this series Professor Murdock presented a survey of relationships between exploitative and settlement features from which certain generalizations were derived. I wonder if it might not be productive to speculate on the sorts of relationships or correlations that might occur between major community pattern types and settlement types.

Before doing this we must determine whether community patterns can be classified in much the same way that Murdock has done with settlement patterns. Can we name five or six general types?

MURDOCK: The literature is clear as to the major community alternatives and their relative incidences. There are patrilocally aggregated groups, matrilocally aggregated groups, avunculocally aggregated groups, and ambilocally aggregated groups.

Patrilocal types of aggregations, whether they are patrilineal or not, are associated with any kind of subsistence economy or settlement pattern. This type is preponderant among pastoral peoples and among those who combine pastoralism with agriculture. Matrilocal aggregations tend to be found in societies with relatively simple agriculture, but there are exceptions. With hunting and gathering economies bilateral relationships are preponderant. In the absence of some evidence to the contrary, the archaeologist would be reasonably safe in assuming bilateral kinds of organization in hunting and gathering societies.

HELM: Our problem is to determine how these types relate to the stability of groups as, for example, the sedentary, the semisedentary, the nomadic, and the seminomadic types.

In this regard, with respect to the western Athapaskans, it is important to note that the people of the Yukon area were aboriginally much more sedentary than were the people of the Mackenzie area, and the former should probably be classed as semisedentary. They also had prominent matrilocal or matrilineal features in their societies. Apparently we can state that matrilocality does not occur in mobile groups.

MURDOCK: The Kaska were quite migratory. Farther west the Carrier were increasingly sedentary. Farther north the Nabesna who lived nearer the coast were also quite sedentary. In general there was a range of more sedentary to less sedentary in this region moving from west to east.

HELM: One statement that probably can be made regarding this situation is that, by all present evidence, a society that is committed to a seminomadic or nomadic style of settlement will not — *ergo*, cannot? — be represented by matrilocality. Conversely, this provides a kind of 'possibilism' argument that the Cordilleran Athapaskans *can* operate matrilocally, although it does not explain why they do.

MURDOCK: Carrying the continuum a step further, on the Northwest Coast proper matrilineality becomes expressed in avunculocal residence. The sedentary life presents the possibility of crystallizing the more amorphous forms of matrilineality. There tend to be aggregations around groups of men, but because they are organized matrilineally it is the males of the matrilineage who are the focus of the group, and they are therefore avunculocal.

DAMAS: This survey gives an outline of some of the associations that we might expect between settlement and community, but are there also alternatives available, for instance, for the sedentary type? Are they predominantly avunculocal statistically?

MURDOCK: Practically all matrilineal people are sedentary. In West Africa the Ashanti and others are avunculocal, and the sweep of Central Bantu across the continent is also completely avunculocal. Toward the east there is an increasing matrilocality primarily because of the initial matrilocal residence that may endure for a considerable period. Regarding shifts from matrilocal residence as among the Bemba and the Yao, the shift is to avunculocality rather than patrilocality.

DAMAS: Can we regard any of these settlement pattern types as being more consistently related to these community types than directly to the economy?

MURDOCK: I would not go that far, but I do think that the compilation I presented earlier might reveal a number of statistically significant correlations between types of descent, lineage, sib groups, etc., and settlement types. There are, however, outstanding exceptions. For instance, the Pueblo are a thoroughly matrilineal people, but the men practise agriculture. There are a number of such exceptions that ought to be considered warning signals, despite the statistical generalizations that I made here.

HELM: This residual category in which a good statistical correlation cannot be found is of interest and probably relates to historical factors.

DAMAS: There would appear to be a lag in such cases, or other determinants may be involved. The following session will be concerned with such situations and with the possible historical factors that are involved.

LEE: Regarding the question of bilaterality and its striking association with hunting and gathering peoples, I suggest that bilaterality is so common not merely because it is one of several sets of rules, but rather because there is an absence of rules. In other words, rules of patrilocality or avunculocality limit possibilities of association. When residence possibilities are limited, for such a system to work there must be fairly substantial permanent groups to receive the incoming spouse. In hunting and gathering societies the groups are too small, too variable, and too mobile and dispersed for the necessary permanent groups to emerge.

DAMAS: With regard to bilaterality, I think it is important to note that this is a large type of classification. With Helm's method of analysis (NMC Bull. 228), variations in group composition are revealed within a range which will probably be considered bilateral.

MURDOCK: From the generalizations I have made regarding the associations of settlement and community types another anomaly can be seen in the Guajiro of the Guajiro Peninsula of Venezuela and Columbia. They are an Arawakan people who had been matrilineal. They became a cattle herding people after contact and are the one outstanding example of cattle nomads in the New World. The evidence that we have now indicates that their aggregations are not patrilocal, as the survey would lead us to expect, but rather avunculocal. Since they had formerly been matrilineal, it is the males of the matrilineage who form the core of the local group. This is the outstanding case in the world of a matrilineal pastoral society, but the factors that cause the phenomenon are clear.

NETTING: There are also matrilineal tendencies among the Taureg, another herding people. Murphy thinks that these tendencies have remained there a long time without change and that they do not seem to be disadvantageous. In referring to the Kofyar, I picked out factors in social organization that I thought might have adaptive value. I did not discuss lineality to any great degree. I think that lineality occurs in what Kroeber calls middle-range societies, but it does not seem to me that the localized patrilineage is of much importance. Household organization as a work group is, I think, important. In my opinion, things might have worked out just as well in terms of survival if the Kofyar had been matrilineal. They could have had individual land tenured rights, just as they do now. Further, it seems to me that for the Kofyar the lineal emphasis is irrelevant. It could have become either matrilineal or patrilineal.

MURDOCK: We must recognize that a matrilineal organization is potentially quite vulnerable in contrast to a patrilineal organization. A number of things can happen to it. It can shift very readily to an avunculocal kind of organization, or to patrilocality, or to bilateral forms. Matrilineal societies lack the momentum that exists in patrilineally organized societies, which contain the seeds for enduring, unless something very serious upsets them. In matrilineal societies, as in all societies, authority rests in the hands of males. There is conflict between the father and the maternal uncle in the matrilineal household. Every matrilineal society has somehow to come to terms with this problem. By contrast, the patrilineal society has a consistency or a unity that gives it a viable value. It will even endure through urbanization. Thus, part of the reason for the predominance of patrilineal societies over the world is their survival value.

RAPPAPORT: There are really two solutions open to the matrilineal society — local endogamy or avunculocality.

HELM: Local endogamy cannot occur in very small groups.

RAPPAPORT: This may be important to the archaeologist. It may very well be that when a settlement, which an archaeologist has reason to believe was occupied by a unilineal group, is below a certain size, it probably would not have been matrilineal.

MURDOCK: That is a good point. Whatever the form of the organization there must be local exogamy if the local group is small. What happens in the case of local exogamy? It means that every marriage brings together a man and a woman from different bands or local communities. It means also that every person is socialized in the cultural traditions of both parents. Diffusion under these circumstances operates through the socialization process, but on the adult level diffusion is imperfect.

The archaeologist can use the following as a guideline: if the community is small, that is, under fifty or even under one hundred, there is bound to be local exogamy and concomitant diffusion over a wide area. The Bushmen and Pygmies in Africa contrast with the Bantu, who came into the same environment with agricultural techniques that enabled them to practise either local endogamy or local exogamy and to thereby undergo local differentiation.

LEE: I agree that Bushman culture is uniform over a wide enough area to contain five Bantu tribes, but it is also the case that an equivalent area would contain ten times as many Bantu as Bushmen, so that the Bushmen must go further to seek mates. A Bantu breeding deme of 10,000 Bushmen would require only 50 square miles on the Zambesi, whereas the same number of Bushmen would require 20,000 square miles. I should add that, although the Bushmen have a highly uniform culture over a wide area, there is great linguistic diversity within the region: four stocks exist, with a very low percentage of cognates between them.

MURDOCK: There are similar situations in other areas, as in California, where hunting and gathering groups have a well-known language differentiation. It seems that in hunting and gathering societies linguistic thresholds do not inhibit marriage.

CARSTENS: When the marriage rules are different, this interaction begins to be inhibited, but generally the probability of intermarriage is a function of the size of the population and the degree of contact.

Factors That Affect Populations

HELM: I think that we should attend to population factors more than we have so far. I refer to Wagley's study of depopulation among the Tapirapé, and its consequences first on the settlement pattern and then on social organization. He found that with coalescence of depleted population segments there were three, four, or five headmen in a single village where previously there had been three, four, or five villages each with its headman.

CARSTENS: Before we can deal with kinship or political authority, we should examine the population and analyse it in terms of economic factors, such as production and the relationship of production to sex and age ratios, etc. This is the base of the total ecological situation.

HELM: One problem of population or demography was raised in Rappaport's paper. He stated that warfare may be the result of limited resources.

RAPPAPORT: There is the question of limitation of resources, but there is also the problem of energy expenditure and return. It is actually a question of whether it is easier to drive someone off land that might be easier to work, or to continue to work one's own land which is perhaps inadequate. It seems to be the case that some conflict is density-dependent, but frequently the triggering of hostilities occurs well below the carrying capacity of the land. For example, if the population doubles in an area, the possibilities for conflict might well more than double. There is an 'irritation factor' that should be taken into consideration.

MURDOCK: The rewards of war and the factors inhibiting it also vary enormously. The Tenino, for instance, maintained peace with the tribes with whom they conducted trade, for they could not afford to wage war with them. They were rivals for trade with people to the north and west, but they had to keep peace with the Wishram and Wasco. If they had not, visiting traders would have resorted to other middle men. On the other hand, the Paiute were poor Basin Shoshonean with nothing to offer in trade and were consequently raided for slaves. The irritation factor is one of the causes of war, but the penalties of war and the penalties and rewards of peace must all be considered in any given situation.

RAPPAPORT: Much of today's discussion points to the fact that certain kinds of community patterns cannot exist without commensurate population sizes. I take it that by 'community pattern' we include not only social organization but also characteristics we regard as cultural rather than social. Among the Maring, the only cases I know of people violating truces were among the largest group living on the most degraded territory at a time when its numbers were swollen by refugees from two other places. I take this to mean that one aspect of the community pattern, that is, the entire ritual development dealing with conflict, was perhaps not sufficient to contain the pressures generated by the increase in size of the population.

MURDOCK: One effect of war on settlement pattern is shown in southwestern Ethiopia, where the entire state is surrounded by a wall. Settlement is dispersed within the wall, but outside there are nucleated villages. In other words, in a situation where warfare is prevalent, nucleated villages provide protection in the absence of a strong state.

NETTING: I think that a population of high density might better be served by being spread out over a greater area, although at times it cannot spread because of the danger of war. One can see this happening in Nigeria. With the elimination by the British of slavery (slavery being the chief cause of war), the Yoruba, who had been stationed in small nucleated communities on hilltops and various other inaccessible places, began to spread down on the plains immediately after the internicine wars were ended. Even in Europe, for example in Italy and Sicily, it was quite dangerous to live outside the walls of hilltop communities until recently.

SMITH: Sicily and south Italy were areas of great *latifundian* estates, and it is only since the central government has had some control over the people that there has been a demand to break up these estates. While the *banditi* were still a powerful influence, the great baron was able to manipulate his forces to protect the peasants. Now that the state offers protection, the peasants want their own land and want to be dispersed.

The Ojibwa are a similar case. There is an increase in natural resources from the northern to the southern part of their area. There was also conflict with the Sioux in the 18th and 19th centuries and a corresponding development of the villages to a much larger size than any found north of Rainy Lake. This concentration of population was accompanied by an increased permanence of the villages and also by the development of chieftainship.

(To Winters): Regarding the nuclear Near East, I have thought that one of the forces leading to the development of city states was warfare. Did warfare indeed have something to do with the concentration of populations into what would become cities?

WINTERS: I think that Jericho would be a good example of this phenomenon. A pre-agricultural hunting and gathering society lived inside a fortified area, where wild grain was grown, which was cut off from access by other people.

There is a similar case in North America. In the Mississippi valley there is not much evidence for systematic warfare prior to A.D. 1000, for it was after that date that the encroachment of non-agricultural people, probably stealing corn from the agriculturalists, encouraged fortified sites. On the other hand, the 'warfare theory' of urbanization is much more tenuous for the Indus valley civilization where the flooding of fields seems to have been responsible for the nucleation of population.

LEE: This discussion of the effects of warfare on the development of civilization in certain cases seems to point to a possible pattern. Population increase, even below carrying capacity, might lead to warfare, and it is warfare that might generate the higher levels of social organization. We previously had thought that only when carrying capacity is reached is there an intensification of agriculture, which sets off the chain of events culminating in the appearance of cities. Warfare could provide the jump in integration well before human ingenuity could devise ways of getting higher yields out of the land.

RAPPAPORT: As Wagley has pointed out, every society or culture has a population policy. Wynne-Edwards suggests that it is very rare for a population to be actually limited by its resource base. There will generally be mechanisms that will cut off population size considerably below the carrying capacity of the land. Where populations are limited by their resources, it is generally through catastrophic reduction of resources and not through an increasing pressure of stable resources. Conflict or warfare may be among these mechanisms. There may be very many others elsewhere in the world.

LEE: We can assume that populations even of hunter–gatherers are always tending to increase, but Birdsell and others have postulated that disasters commonly occur only once a generation or so. Accordingly, a 'saw tooth' pattern appears; there is generally a period of slow growth (the steepness of the curve of growth depending ultimately on fertility factors and infant mortality), with the growth period being followed by a sharp drop, after which the population resumes the upward swing again.

MURDOCK: But even the peaks are a long way from the potential carrying capacity of the land.

LEE: That is because the population is being regulated not only by food availability but also by density-independent factors, such as epidemics. Features of social organization or community pattern are also affected by population factors. Take, for instance, the question of bilaterality versus unilineality. In very low population densities bilateral groups are the only possible ones existing, because unilineal descent groups appear to be beyond the limited demographic means of very low density societies.

BICCHIERI: I think we could make a fair statement as follows: 'population down, rules down' because alternative solutions must be available, i.e., channels must be left open. Conversely, when the population is up, the channels can be closed.

MURDOCK: There are Australians whose local groups are made up of a single patrilineage to a camp.

HELM: But Hiatt feels that there are many bilateral linkages among the people who actually aggregate, not necessarily those who belong to a piece of territory but those who exploit a resource together.

LEE: There are some Australian hunters and gatherers who have substantial environments which could support high population densities. It is not only a question of evaluating the environment but also of evaluating birth rate. What is the average family size per woman? What is the probability of a man having a son; and if he has a son, what is the probability of his maintaining a core of patrilineally related males? Where patrilineages exist, we would expect to find birth rates high enough so that enough sons can survive to maintain the patrilineage as a normative system. However such people as the Bushmen or BaMbuti have smaller populations at lower densities so that groups are made up of whoever is available.

HELM: This discussion has proceeded from a consideration of relationships between settlement and community patterns to warfare and, correlatively, to traits of population structure. It appears that these population factors of growth, density, and controls may indeed be crucial in shaping features of community pattern without necessarily being dependent upon settlement pattern features. Important, too, have been statements that population factors are not always directly the result of environment or exploitative level. I think that while the discussion has shown that there are frequently correlations between either exploitative or settlement pattern with community pattern, a simple direct causal chain cannot be extended from exploitation to settlement to community or social organization. Although we have focused upon explanations for social features that may be related to adaptation of various kinds, we should not expect to relate all such features to external systems. Adaptation or ecology cannot answer all questions about the occurrence of specific features in specific societies. Other approaches are needed to supplement the ecological approach in many cases.

HISTORICAL FACTORS

BY

EDWARD S. ROGERS

Introduction

I have been asked to chair this session on the relationship of historical factors to social features. It would appear from earlier discussions that ecological analysis can account for certain features of social organization, that is, an explication of the relationship between social features and a given exploitative and settlement pattern, but there are certain anomalies to such explanations. Ecology might account for gross social features but not for minor ones.

Before considering the anomalies, we could perhaps mention those features which can be designated the 'culture core.' This complex is generally considered as those key features within the society that regulate or 'determine' other features of social organization. If there are to be cross-cultural comparisons it does not seem possible to have different types of 'culture cores' to compare.

The second problem I am concerned with is: what exactly is cultural ecology? I have always thought of cultural ecology as the interrelationships, established by a people's technology, between the natural and man-made environments and the culture of the people. But I find it difficult at times to see how this interrelationship operates.

Whatever the answers to these questions might be, we are still left with certain anomalies. I should mention some examples of social features that cannot be explained in terms of ecological analysis.

First, I will mention Damas's case of three Central Eskimo groups which appear to inhabit a uniform environment and to possess similar overall cultures but which display differences in kinship terminology and differences in social units, namely, nuclear families in one area and extended families in the others.

Secondly, in the Ojibwa area of northern Ontario the gross features are uniform throughout, but there are minor differences among contiguous groups. I am thinking of one group in northern Ontario whose members do not behave formally toward in-laws but treat them as equals, while immediately to the north members of another group treat their in-laws deferentially, having different terms of addressing them, etc. I cannot see any environmental explanation for this. A difference can also be noted in the varied terminology for mother's brother. In one area he is called 'little grandfather'; in another the term is 'little grandmother.' Other examples of terminological contrasts could be cited.

Another anomaly is found on the Plains, where people moved from the Missouri Basin onto the high plains and have retained the Crow kinship system. Professor Murdock has mentioned Indians of the Panama area who adopted cattle-herding yet maintained avunculocal residence, an anomalous mode of residence for herding peoples. In addition, there is the case of the Western Pueblo who have matrilocal residence, yet agricultural work is done by the men. We might ask Rappaport whether there are differences in terminology or other kinship features in the New Guinea highlands that cannot be explained in terms of ecology.

The question we now must ask is: what historical factors might account for these anomalies which cannot be explained by ecological analysis? I will suggest several of these factors. One is the *diffusion* of social traits from one group to another. Another is *divergent drift* or slow change over time in the social organization that might occur, for example, between two groups which have split from a single group with a common origin. A third factor is *historical accident*: for example, forced social change which is not contingent upon the natural environment. This factor is particularly apparent in my own work on Indian reserves that have been hit very hard by pressures from western society and have consequently been forced to change social institutions. Finally there is migration of groups from one environment to another with *cultural lag* operating to maintain certain traits, that is, a social institution appears in a particular situation where there has not yet been time to adapt to the environment or to a new exploitative pattern.

In the past, there has been disagreement regarding the possibility of the diffusion of social features. Murdock (1949) and Eggan (1955) would allow for the diffusion of cultural traits but have thought that social features change mainly internally. That is, they are relatively independent of the effects of contact. In Murdock's scheme of evolution, division of labour is basic and leads to a definite residence pattern, which in turn leads to a particular type of descent system and to a kinship system. If the division of labour is altered, other features of the system also shift. On the other hand, Driver and Massey (1957) accept evidence for this process but also feel that there can be diffusion of social as well as cultural traits.

As I have mentioned, not only diffusion but other historical processes may also account for anomalies to ecological explanations.

A possible example of *diffusion* is that of the Plains Cree and the Plains Ojibwa who moved onto the Plains and rather rapidly adapted the whole Plains Complex or Configuration. This change took place in a very short period and subsequently raises the question of the length of time needed for indigenous adaptation of culture and society to a particular environment. If it takes 1,000 years for this change to take place, then it is clear that the Ojibwa and Cree obviously 'borrowed' the whole complex. If it takes only 50 years, then it is possible that they evolved in that direction indigenously without contact having been necessary with the Assiniboine or Blackfoot or other Plains groups. Perhaps we could determine through historical reconstructions whether intermarriage had occurred with other Plains tribes which effected adoption of Plains culture.

There is, in addition, the process of *drift* that may give rise to local variations. Great problems are involved in finding convincing evidence for this process. For example, snowshoes occur only in association with snow-covered areas. We try to see a cause-and-effect relationship here since we know that snowshoes do not exist in the tropics. However, the causal relationship is not simple and clear-cut. The problem then is: do the existing varieties of snowshoes relate to underlying environmental differences, or are they products of apparently random divergences arising from a presumed common origin? Furthermore, in terms of *drift*, the Eskimo present a case where apparently random divergences arose from a presumed common kinship system. *Drift*, a slow process over time, operating under conditions of isolation, can at times conceivably be argued to be adaptive and perhaps related to micro-adaptive features.

My case of the reserve Indians is an example of *historical accident*. The Hottentot material might also give illustrations of this form of process.

With regard to *cultural lag*, I have mentioned the case of the Crow who moved out onto the Plains and retained the Crow kinship system, which is not fully in accord with the rest of the Plains groups (though there are some groups to the south which do have this system).

The question then is: what are the mechanisms that make it possible for these specified factors to operate? What, for instance, are the mechanisms for diffusion of social traits; are they intermarriage, warfare, or slavery?

In summary, we are confronted with the problem of trying to isolate social features which do not seem to be explicable in ecological terms. For example, if a similar social feature is found in several habitats, it could indicate a *common heritage*. Moreover, if the feature is assumed to be non-adaptive in one or more of the habitats, we would also assume that *cultural lag* is operating. Furthermore, if diverse social features occur in a uniform environment, they would not appear to be ecologically determined. If, for instance, there is a social feature that appears in different forms, and if on the margins of the area there is contact with a similar social feature, diffusion could be indicated; if there is no evidence for such contact, *divergent drift* could be the postulated reason.

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DISCUSSION

The Problem of Variation

NESBITT: Rogers has raised the question, "Did the existing varieties of snowshoes have a common origin?" I raise the question, "Is it not possible that the varieties may have an ecological basis?" Arctic snow runs a gamut of types — dry-packed, wet-packed, powdered, drifting, etc. There may be as many as a dozen differentiated types. Are there any correlations between snowshoe varieties and types of snow? Perhaps this suggestion is worthy of investigation.

ROGERS: Unfortunately no such work has been undertaken, but perhaps now is the time to give serious consideration to making such an investigation.

RAPPAPORT: In response to Rogers' main point about historical factors and social features, I would like to mention that Meggitt found that the eight-class system in Australia has diffused recently.

With regard to Rogers' question of variation in New Guinea, one finds a whole set of shared ritual elements in the Eastern Highlands, but these ritual elements are arranged differently in time and space among different people. If anthropologists had carried out ecological rather than conventional social anthropological studies, I am sure functions similar to these of Maring ritual would have been discovered to be associated with ritual elsewhere in the Eastern Highlands. These variations cannot be related to any gross differences in the environment, but careful studies of the soil, climate, crop inventories, etc. would be likely to expose subtle but significant differences. There is a great opportunity for a microevolutionary study in the area.

BICCHIERI: There is the case of the food-collecting Dorobo and the neighbouring pastoral–agricultural Nandi of East Africa. Huntingford indicates that through historical contiguity the Dorobo have acquired the formalized class system (*Koret*) of the Nandi. Close cultural–ecological analysis suggests that even though the Dorobo have adopted the overall *Koret* terminology and structure, they have, nevertheless, adapted it to their 'cultural core.' For example, among the Nandi selection of personnel is kin-based, whereas among the Dorobo, because of the dispersion of the household in response to isolative, physical forest conditions and type of economy, selection is concerned with household representation.

DAMAS: Do you relate these variant principles of selection to adaptive forces?

BICCHIERI: I would, as has been the general agreement in this conference, although there does not have to be the suggestion that ecology should be looked at in a deterministic sense. There are probably other kinds of factors as well that are responsible for the variation.

DAMAS: I suggest that many of these factors can be subsumed under 'historical processes.'

Ecology, History, and Ideology

CARSTENS: At this point we seem to be mainly concerned with forces of change from the point of view of historical factors. Does this mean that we are abandoning the ecological approach?

HELM: Not really. There is a fairly direct analogy to biological evolution in this case. Ecology is being handled as adaptation or natural survival, the Darwinian aspect. All these other factors — diffusion, drift, historical accident — fall into a genetic aspect like mutation, gene flow, etc.

RAPPAPORT: The thrust of this argument is that there are not two different kinds of explanation. There is not an opposition of historical and ecological explanations; they complement one another.

One of the problems involved here is an attempt to make a direct correlation between the formal features of a particular component and the environment in which it occurs. Using a biological model, the formal aspects of an organism, that is, how it will adapt to a particular environment, will depend not only on the environment but on the nature of the organism at a previous time. I do not think that we are going to find close correlations between particular features and particular environments because the fate of a society will depend not only on the environment but also on what was brought to the environment. One must define the unit that occurs in a particular environment before an ecological approach can be used.

ROGERS: I am wondering about the importance of personality, the value system, in this context. In the case of the Ojibwa, who have an elaborate system of witchcraft, some groups moved out on the Plains and lost that feature. They changed their personality or values. This suggests that there are situations in which personality features may either be carried into a different area or may be relinquished upon entering a new one.

HELM: We might hypothesize that there is something about the northern boreal forest that stimulates a frame of mind that gives rise to witchcraft, and that this is lost when the people move to another area, lost because of environmental reasons. However, with the Déné, the Algonkian kind and intensity of witchcraft does not seem to exist, even though the Déné live in very much the same kind of boreal forest as do the Algonkians in the east.

ROGERS: I think that it is simply a matter of an Ojibwa propensity to elaborate witchcraft.

RAPPAPORT: There is no reason not to take personality variables into account. I would state, however, that we should not expect to find much correlation between the cultures of the Bushmen and of the Australians, even though they live in highly similar environments. The cultural material brought into the two similar environments was no doubt different in many respects.

BICCHIERI: What you have been discussing is really description, not system, because before we have a system we must describe the population that occupies the environment. Adaptation must operate on some sort of organism. One must determine its physical nature, whether it is tall or short; what its historical background is, etc. This is the descriptive part of the study.

The Problem of Alternate Solutions

LEE: Perhaps we should consider the question from the perspective of social solutions to adaptive problems. Every society has to behave in a sufficiently business-like manner to ensure survival; but not every society, after all, is going to exhibit *the* optimum adaptive pattern for a given environment. For example, family organization may vary from nuclear to extended without significantly affecting the society's ability to meet caloric demands.

DAMAS: This problem of minimum solutions has stimulated my concern with ecological problems on the one hand, and historical-distributional problems on the other. It is true, as you note, that there are certain shared traits, especially economic practices, which seem to be limited by what the environment offers. I will have to modify this, however, when I recall the *uuttuq* sealing situation, as noted in my paper. But more germane to the present discussion, most of us had accepted that in the case of the Eskimo there would be the minimum of social solutions to problems posed by the environment because of their low population density and low level of subsistence. However, when I found alternative social features in ecologically uniform areas adapting to similar situations, I felt that if there are that many alternatives at a low population level and low subsistence level, then there would be many more alternatives at other levels.

CARSTENS (to Damas): With regard to the residential material you presented, which showed variation between nuclear and extended families, is there really much difference in family organization in the three regions? Perhaps most of the societies that have been characterized either with the nuclear or the extended family really have a range of about 40 to 60 per cent, which might vary back and forth over time.

DAMAS: If the difference were only of the 40 to 60 per cent magnitude, I would agree, but I have reason to believe that perhaps 90 per cent of the Netsilik or Iglulik nuclear families related to a larger sharing and residential unit in a consistent fashion, whereas among the Copper Eskimo the percentage would probably be about 10 to 15 per cent.

HELM: The test in the final analysis is, which system has survival value, which is efficient? Since no one actually is allowed to starve if someone in the camp has food, we can probably say that both have sufficient survival value. It is only in the formal features of distribution or eating practices that there is actual variation.

DAMAS: Either system can work; nevertheless there are differences in organization. Should not these differences be explained?

HELM: Perhaps we have tried or expected to find too narrow an adaptive relationship. The Eskimologist at least should try to explain these variations, but at the general level we can expect categories of variants within a range.

DAMAS: The reason that I seem to be encountering problems of this sort, while others do not, may be that there is a peculiar situation in the Eskimo area where one people sharing a common heritage has spread, relatively recently, over a large, very sparsely populated area. This condition is unfavourable for intergroup contact, which in turn encourages marriage isolates (as noted by Carstens). Conditions of isolation develop very quickly. If there is any situation that encourages cultural drift, it would be this one. There are only a few places like this in the world, the boreal forest probably being one of them.

SANGER: We must consider the factor of experimentation in culture. Time depth must be considered when we are concerned with environment or adaptation in our studies. Is it not possible that any one group can operate within a specific range of types, and as a cultural/social anthropologist in 1960 you see it at one level? Why could the features not vary from time in any Eskimo society, and from one society to another, without it implying any significant difference in ecology?

DAMAS: I would expect this to be true. (To Helm): Returning to my comments about the apparently unique situation of variation among the Eskimo, what can you say about the distribution of kinship terminology in the Athapaskan area?

HELM: The Hare possess a perfect Iroquois-bifurcate merging terminology, while the neighbouring Dogrib have a Hawaiian system. For both groups, caribou hunting is the main subsistence activity, augmented by fishing. I do not see any ecological differences that would account for this variation in terminology, though I may not be attuned to microenvironmental differences. Perhaps we have tried to find too narrow an adaptive relationship. For example, one might find both the extended and the nuclear family domestic unit in the Eskimo area, but not the Iroquois longhouse type of residence organization.

RAPPAPORT: It is logically indefensible to maintain that there would not be a range of variation in these matters. You would have to demonstrate that there is no functional alternative in a particular situation. To do so is almost impossible.

The Limits of Ecological Analysis

ROGERS: Only future research will demonstrate which social features might be considered adaptive. A lot more intensive work must be done. At the same time I wonder if there are not certain limits within which there are pulsations. We should try to isolate those features which are adaptive before we begin to work with those which are relatively unaffected.

RAPPAPORT: By social features I understand that you mean something with certain formal characteristics. I am not sure that I sympathize with an effort to focus upon adaptive versus non-adaptive features. My feeling is that almost any feature could be adaptive. What there should be is an analysis of a particular situation. In any particular situation there is going to be some formal characteristic that is adaptive in a very specific ecological sense while in another region or at another time the same formal characteristic may not be adaptive.

If an attempt is made to determine origins, I am not sure that it can be made from the approach of ecological analysis, that is, a comparison of adaptive systems from society to society. If we encounter a patrilineal organization in a particular society, all we can say is that, given this patrilineal organization, there are the appropriate aggregates of men, the recruitment of adequate work forces, the occurrence of a balanced sex ratio, etc., but we cannot say that this is the only structure possible under these conditions.

DAMAS: It occurs to me that if we acknowledge (as seems to be implicit in the foregoing discussions) that origins cannot always, generally or even frequently, be determined through an ecological approach, this circumstance places a very severe limitation on the ecological approach.

Rappaport: True, but we may as well be aware that we are not going to be able to get from ecological analyses to explanations of the origins of formal features by any easy step.

Helm: We can, however, determine the kinds of structural features that tend to be associated with sets of ecological variables.

Rappaport: Yes, but if we try to explain presence of origin of features, we must consider why, given similar relationships amongst variables in the areas, the features do not correspond 100 per cent in the two areas.

Helm: Still, I feel it is possible, or at least desirable, to try to determine a focus of ecological features, around which social or ideological features revolve.

Lee: Carrying capacity may determine the number of persons that can be supported on a given range, but it may not predict whether these persons will cluster in a few large groups or in many smaller ones. Bicchieri's discussion of conflict and of cohesive and disruptive forces may be applicable here. Is it not possible that societies will vary in the degree to which the members will tolerate or avoid social contacts? The Bushmen, for example, live in small groups, with as many as five camps of twenty individuals at a single waterhole. !Kung informants were asked; "Why not live in one camp of a hundred people?" Their reply was, "We live in small groups because we fear fights." This is a case where ecological features may set the limits of a population, but the ideology of conflict accounts for microvariations in group size.

Helm: But may not that situation be ultimately a function of the exploitative pattern? For instance, the Maring cannot decide to spread over a large area. Their habitat has limited them but has allowed or permitted them to make a certain set of spatial arrangements.

Lee: That is true, but much is left unexplained within these limits.

Rappaport: Certain kinds of exploitative situations imply certain densities of population out of which certain problems arise, such as a degree of intragroup conflict. Such problems, then, may require one or more sets of means for handling them.

An ecological framework does not restrict us to the classical relationship of 'man–resource base.' Also, the total adaption will involve features related to social factors.

Rogers: We will probably all agree on one point: Man needs food and is therefore going to have to exploit his environment to get enough calories to survive and reproduce. The exploitative patterns will vary; this means that there will be variations in the organization of his operation and his control of the environment. We have been talking about interpersonal relations and conflict and group size. Do we have a need for a minimum group size for interpersonal relations, and is there also a maximum above which people will have to separate or have mechanisms whereby they can reduce the tensions? Does this have any relevance to archaeology? Can we use archaeology to show that there is evidence for this need for minimal and maximal group sizes? Or does group size relate to cultural–historical factors such as historical accident or features like witchcraft? The fragmentation of the Ojibwa groups is related to witchcraft, yet at certain times they have to come together in order to fulfill a need for interpersonal relations.

HELM: There is the dimension of cultural drift. Human beings are capable of enduring stresses in maintaining certain institutions even to their own psychic and physical cost. It is very difficult to handle such situations within an ecological perspective or to read functional explanations into such situations. Sometimes cultures do allow areas of florescence. They allow experimentation, and sometimes they develop an attachment to their own experiments. No other explanation seems possible except that certain features are cultural–historical developments. This situation is not amenable to a functional explanation.

Summary: Ecology and Historical Approaches

DAMAS: As we have talked we have seen that social factors intrude on all levels — exploitative, settlement, and community. Rappaport pointed out that formal features seem in themselves to be 'given' facts which enter into this nexus of adaptation. These features would probably comprise largely what we have regarded as community pattern. Can we say that community pattern features are those which are most likely to be influenced by non-adaptive or purely historical factors?

HELM: They could be compared to exploitative and settlement features.

DAMAS: The question that I wish to pose at this point is the following: Is examination of the historical factors a legitimate aspect of ecological studies, or do we complete our study before considering such factors?

RAPPAPORT: If one of the objectives of a study is to explain the presence of a feature, then I think we need the history of the situation, and in this case it is a perfectly legitimate part of an ecological study. A functional explanation can consider the components of a system at a particular time but cannot explain the occurrence of the components of a system. If one wants to explain the presence of a trait, one must consider its antecedents.

DAMAS: A conventional social–structural study that concerns itself with the description and with the integration of the social system itself is circular. An ecological study enables us to break out of the circularity to a certain extent, but it is in itself merely a larger circle.

RAPPAPORT: This should not trouble us.

BICCHIERI: One example of historical factors in operation is given by Turnbull. He suggests there were historical factors relating to historical contacts between the BaMbuti and the Negroes that established certain groups as archers rather than net-hunters. The environment was permissive enough to allow them to use alternate economies.

HELM: If there had not been any BaMbuti archers, it would have been possible to work out a seemingly sound explanation of net hunting as the only possible way for a pygmy to survive in the Ituri forest.

DAMAS: This example raises the problem of just how much we can do to establish adaptive correlates if we restrict our study to one group of people. Cultural ecology is an area of study that emphasizes the importance of comparison on large and small scales.

LEE: However, we should be careful to compare cases with adequate data so that we can learn more about dynamics to be able to ask better questions.

RAPPAPORT: We must seek new units of comparison for such studies; the formal characteristics of social systems probably do not provide the bases for such comparison. Rather, I suggest that a comparison of ecosystems and associated populations might provide such bases.

HELM: It may well be that comparisons of features like patrilocality, matrilocality, etc., may simply be artifacts of our traditional means of analysis and our principal descriptive concern in the past. We will have to try to determine how these formal features accrue to certain complexes of ecological variables, but in doing so we may find that we are considering a range of variations or alternatives which, accordingly, makes the specific feature no longer distinctive.

DAMAS: I would like to point out, however, that as our study moves further from these formal features we are at the same time moving away from the conventional and traditional concerns of the social anthropologist. In addition, if the full explication of the ecological factors is achieved, as much of the foregoing discussion seems to imply, and the occurrence or distribution of many formal features remains unexplained by this ecological approach, then the social anthropologist must resort to historical processes and historical approaches if he seeks explanations of origins.

RAPPAPORT: At that point the study could continue or end, according to personal taste. I would be content to state merely that, given similar relationships among variables, a certain group is patrilocal, another is matrilocal, while still others are bilocal. Other anthropologists will want to explain this variation, and theirs will be the difficult task of seeking out the antecedents in the alternate situations. I think that this is a very worthwhile but also a very difficult endeavour.

METHODOLOGY OF ECOLOGICAL ANALYSIS

The Ecosystem

RAPPAPORT: I think that it can be rewarding for human ecologists to use units similar to those used by animal ecologists. This procedure enables them to work with broader theory than our own without requiring the use of analogies. Groups like the Tsembaga, for instance, can be regarded as populations in the ecological sense without violating sociocultural or ecological concepts. Their territory, similarly, can be regarded as an ecosystem. Naturally, analysis like this is not always so clear-cut. It might be convenient to regard the territory of an atoll population, for instance, to include three ecosystems—the land, the reef-lagoon, and the open sea.

NESBITT: I agree with Rappaport that an atoll comprises three different ecosystems. True, man is a predator in both the reef-lagoon and the open sea, but he differentiates between these two ecosystems, and it provides the frame of reference for certain basic postulates that govern his thinking and actions. The sea, unlike the reef-lagoon, is an area of danger and insecurity. Expeditions into it are carefully planned and well organized. This means the preparation of canoes, the accumulation of food and water, the knowledge and use of numerous rites and magical spells, etc. No such preparations are necessary in the comparatively safe reef-lagoon. Malinowski's "Argonauts of the Western Pacific" points out the importance and profound differences of the ecosystems in the pattern or design for living.

MURDOCK (to Rappaport): If we regard the atoll as three ecosystems—land, reef-lagoon, and sea—is this not a product of a map fallacy? We should try to devise an ecological definition of the ecosystem, not a map definition.

RAPPAPORT: I agree that in reference to our particular research problems, we should not succumb to map fallacies but should discriminate units of study, such as ecosystems. It is in this respect, however, that I suggest that it *might* be illuminating to consider the atoll as composed of three ecosystems, even when our focus rests upon a single human population that participates in all three, because the associations of plants and animals occurring on the dry land, in the lagoon, and in the open sea differ from each other both in composition and in man's position in them. On the dry land, man is the dominant species, setting the conditions that encourage or discourage the presence of other species. He is dominant neither in the lagoon nor in the open-sea ecosystems. In the lagoon he is a generalized predator of many species of plants and animals; in the sea he is a more specialized predator, concentrating on capturing a few animal species.

ROGERS: From a biologist's point of view, the atoll could be considered three ecosystems, but from the human point of view a common environment is being exploited.

RAPPAPORT: I agree that the atoll could be, and perhaps would have to be, treated as a single system. The land, the lagoon, and the sea are all exploited, as Rogers notes, by a single human population. Furthermore there are non-anthropocentric reasons for regarding the whole as a single system: ecosystems are more or less distinct material-exchange systems, but none are completely independent of those adjoining them; there are always exchanges across ecosystem borders (which we designate somewhat arbitrarily). An example of this exchange is evident in the manuring of the land by birds which feed in the lagoon and sea. I am suggesting, however, that ecological analysis might be facilitated by recognizing, in the case of the coral atoll, three more or less distinct ecosystems as subsystems of a more inclusive system.

HELM: We might take the example of people who fish at a lake for four months and then move to the barrens to hunt caribou. Are these separate ecosystems? From the point of view of the population, at least, the ecosystem has to include the total range of its environmental milieu.

MURDOCK: In the cases of the Hausa and the Fulani, there were two human populations in one ecosystem. In the case of the atoll dwellers, there was one human population extracting its livelihood from three different sets of biota. This implies that there can be a two to one, or three to one relationship between the human group and the kinds of resources.

HELM: The ecosystem is defined for human groups as culture bearers, a condition which makes them distinct from any other life form. Part of the environmental definition (e.g., exploitable resources) must derive from a cultural definition (e.g., exploitative techniques). In 'the study of man,' the ecosystem should be phrased in anthropocentric terms, i.e., in terms of the human, culture-bearing population.

RAPPAPORT: The conceptual question being raised here, I think, is whether or not we should use the term 'ecosystem' to refer to the 'total' environment of the human group. The total environment of any human group includes everything that impinges upon it in any way. The term 'total environment' or even 'environment' thus does not imply a discrete unit of any sort. I would be reluctant to use the term 'ecosystem' in such a vague way.

NESBITT: Could we not use the word 'microecosystem' to mean the territorial ecosystem and 'macroecosystem' to mean the territory beyond that which is normally utilized?

RAPPAPORT: I think that Nesbitt's suggestion is valuable, but I disagree with his choice of terms.

I would suggest that for analytical purposes the 'total environment' is amenable to two cross-cutting distinctions. The first is the distinction between what may be called the 'immediate' and 'non-immediate' environments. The immediate environment consists of that portion of the biosphere from which a particular group derives its subsistence; this may be treated as one or more ecosystems. The non-immediate environment consists of entities with which the population interacts, but which are external to the immediate environment. Other human groups are likely to be the most important components. Typically, the exchanges that take place in the immediate environment are interspecies trophic exchanges, while those taking place with reference to the non-immediate environment are intraspecies exchanges involving personnel and trade goods.

Cross-cutting this first distinction is one between the 'operational' and 'cognized' environments. The operational environment is composed of those phenomena which an anthropologist determines, by empirical procedures, to be impinging upon a human population. It is the anthropologist's model of the real world. The cognized environment consists of whatever entities a people understands to exist in its surroundings whether or not they can be shown to have material existence. This is the people's model of the world, and it is presumably the model in terms of which they act.

NETTING: I think that the biologists would be unhappy to limit the ecosystem to the immediate interchange of organisms in their environment, as we have been doing. Certainly they would include the interactions of organisms, even the species, in their ecosystemic analysis, and if we wanted to take the largest definition of ecosystem we would also have to include relationships of antagonism among the local populations.

RAPPAPORT: This is certainly the case. Ecosystems can be descriminated on any scale of magnitude. My suggestion is one concerning methodological strategy. I am suggesting that to focus upon interspecific material exchanges (the exchanges involved in subsistence), we discriminate units that are manageable in size, which may also be the units most directly involved in these exchanges. The discrimination of such units — local populations and ecosystems — permits us to begin to quantify data with some degree of precision. I am not suggesting that we ignore the relations, antagonistic or otherwise, between local populations. In discriminating what I have called the non-immediate environment of the local group, a more inclusive human aggregate emerges. This may be termed the 'regional population,' an aggregate composed of the several local populations that occupy a larger, more or less continuous region. Consideration of relations among local populations within the regional population is, it seems to me, an intrinsic part of ecological analysis.

MURDOCK: It is possible to use a larger area as an ecosystem, but in doing so we cannot use the kind of analysis that we are trying to develop here. We should probably thus speak of the 'minimal ecosystem' and not deny that there are others as well.

LEE: This conception agrees with modern ecological thinking. Former community ecologists, by using 'energy exchange,' tried to encompass all transactions between species in a community. However, by taking a population approach and focusing on one species, man, we are able to define external and internal relationships more adequately.

RAPPAPORT: One of our aims is to discover the place of the human population within the ecosystem in which it exists. In the analysis of the ecosystem relations of a particular local population, our starting point would probably be the material exchanges (with other plants and animals) in which the local population is directly involved. We then might want to look into the question of what the plants and animals, upon which man subsists, depend upon themselves. How far we would trace these dependencies (which will correspond to a large extent to food chains) would be dictated by our research problem. I would suggest that ideally we would want to trace them back to the factors that are limiting, in the final analysis, to the human population.

NETTING: There was disagreement about this point in one of the Columbia seminars on ecology, which was attended by a number of zoologists, botanists, and animal ecologists. Klausberg gave a paper about the definition of an ecosystem, but the discussion concluded with a very operational definition. Our important task is to define the particular universe we wish to use. We can, if we wish, make it larger or smaller, but the system we have been talking about is manageable enough to work with.

The Ecological Population

LEE: Given this operational definition of the ecosystem, our next step is to define the population that inhabits it.

RAPPAPORT: The animal ecologist usually defines the population merely by species designation because, as far as most animals are concerned, taxonomic designation implies an ecological niche. This is not the case with respect to humans. The crucial thing, in ecological analysis, is not the taxonomic position of a population, but its position in the biotic community. I would define a population, human or otherwise, in the ecological sense, as an aggregate of organisms sharing distinctive means for maintaining common trophic relations in the ecosystem in which they participate together. The criterion, then, for discriminating the ecological population is its niche, which is defined by a particular set of trophic relations and by distinctive means for maintaining these trophic relations.

HELM: Using this definition, I suppose one could argue that a household is an ecological population in that the resource base, the use of a single piece of land, could be restricted to that unit. In the Déné area, one household brings in small game and fish, and unless someone else is starving, they eat their own catch. The big game, on the other hand, is shared beyond the household.

RAPPAPORT: I think that probably in many societies with paleolithic or neolithic modes of production, the household is the most significant unit not only with respect to the extraction of materials from the immediate environment but also with respect to the consumption of them. It is usually the case, however, that the household is imbedded in a larger local group, such as a village or band, with inherent claims to the resources found within a bounded area. I suggest that in simple food-producing and food-gathering societies the household is likely to be our unit of observation so far as extraction and consumption are concerned, but that our analytic unit, the group which we would designate the local or ecological population, is the more inclusive territorial unit. This is likely to be the local community, for in neolithic and paleolithic societies such groups are likely to claim access to the resources occurring within a demarcated area. Conversely, the territory of the local community and the animals and plants occurring within it may be treated as one or several ecosystems.

HELM: We must eventually compare what different ethnologists call a population. In the case of Déné a local population is gathered tightly together but may not have duration of more than three months. Is this to be my population, or is it the regional band which may never entirely aggregate? If the people are dispersed much of the time, they are sharing a general domain, but they are not in interchange with one another with the resources.

RAPPAPORT: They are sharing access to the resources occurring within a bounded area. Whether they share the resources after they are extracted does not matter.

HELM: Those of us who work with band societies have the problem of coalescence and fragmentation. When the bands are fragmented into two or three family groups, are we watching these fragments or are we still treating the whole band when we consider the question of a population?

Rappaport: We should, I think, still treat the whole band as the local or ecological population, since the whole band, whether it is coalesced or fragmented, shares the resources of the territory. This strategy permits us to estimate densities and man : land ratios. On the other hand, we may have to focus upon one or another of the fragments with respect to the collection of data concerning actual extraction. But this data, with some control and adjustment, might serve as a sample for the entire band.

Rogers: The problem that I have in the Algonkian area regards the hunting groups which disperse for the winter and later aggregate into the band for the fishing season. By putting together their regions of utilization, there is a rough area for the boundary of this larger group.

Helm: The situation is similar in the Athapaskan area. The local group might aggregate at the same place every year, but it does not have to. I feel that I can handle this territory concept only on the level of the regional band. In addition, with the regional band, if it is necessary and if there is sufficient population, the group can maintain itself by endogamy. The regional band can be regarded as a marriage universe, though usually this does not seem to be the case.

Rappaport (to Helm): From the foregoing I would suggest that your regional band corresponds to my local population.

Furthermore, you appear to have a situation in which the regional population and the local population are coterminous.

Bicchieri: Here is an essential difference between food collectors and agriculturalists. In the latter case the local group is usually large enough for marriages between members to take place within the settlement.

Rappaport: Confusion is likely to arise in cases like Helm's. The application of two sets of criteria yield the same unit. Here the ecologist's population and the geneticist's population may be coterminous.

Lee: There is an important difference here. In the case of Rappaport's agriculturalists in New Guinea, it might be possible to define the limits of the ecological population by the rate of flow of foods in the same way that a geneticist defines a breeding population in terms of gene flow. If there is a reduction in energy flow to perhaps 10 or 15 per cent, that point is the division between two populations. In the case of a hunting and gathering population, between camps there is a minimum flow of food, or rather of people, and interdependence in an area is to be seen in these terms.

Quantifiable Variables

Lee: If we wish to compare ecosystems and population characteristics, we will need categories of comparison. Can we agree on some of these? I can suggest population size or *density* as one. Another would be caloric intake or *energy measure*.

Helm: We will also have to consider *spacing* of population, how the density is handled.

Bicchieri: We must also gauge *exploitative efficiency*.

ROGERS: We have spoken of one species, man, but there are variations in size of men and in their susceptibility or tendency toward certain diseases. The dimension of the *physical population* must enter into our equation.

RAPPAPORT: This is an important point. Anthropologists are inclined to regard the members of various human populations as interchangeable. If we were animal ecologists, we would want to specify the population in terms of the demands it makes upon its environment. The kind of approach we are developing here requires that we specify aspects of the human population as rigorously as we specify aspects of its environment. The physiological requirements of a population of humans who weigh 100 pounds each will no doubt be different from the physiological requirements of a population in which average weights are between 150 and 160 pounds, and it is reasonable to expect that populations distinguished by such differences would make different demands upon similar ecosystems. And in addition to differences in susceptibility to diseases which Rogers has mentioned, there are possible significant differences in the extent to which the members of different populations can tolerate climatic and other environmental conditions. What is indicated here is a newer and tighter integration with physical anthropology. I hope that physical anthropolgists will begin to develop more physiological, rather than strictly genetic, interests.

HELM: I think that standard of living must also be specified in terms of what exists in a particular system. Some peoples feel that they require gabled houses; therefore a great many man-hours must be devoted to acquiring them. Correlatively, this means that a number of people have to be taking energy out of the environment to feed a number of people who are not taking energy out of the environment but are using their energy, instead, in building the house. The total energy taken at any slice in time maintains the standard of living as it exists; this, more properly speaking, should be termed the *level of living* (as against the normative 'standard').

LEE: Here we should distinguish between self-contained and interlinked energy systems. Consider the example of a peasant agriculture system that is managing nicely when it is taken over in conquest by a large state which proceeds to exact one-third of the crop as taxation. A new element in the ecosystem has appeared, and as work expands, caloric intake decreases. Another aspect is present in hunting societies, for instance among the Bushmen, where the men hunt and the women gather, because they have no alternative. Whereas, when the Dogrib's system cannot support them they can go to the fort and throw themselves at the mercy of government welfare.

ROGERS: I would like to add *numbers of individuals in units* to our list of variables.

RAPPAPORT: What we have been doing is noting possible variables which are amenable to quantitative treatment. The significant ones, of course, will vary from situation to situation, but I would suggest that other possibilities might include population structure, that is, age–sex composition, and population growth rate; and in the environment such variables as territorial size, amounts of arable land, yields of useful biomass per unit of area per unit of time, and so on, should be considered.

Other Variables

RAPPAPORT: Thus far we have been concerned with variables that extend through a continuous range of values. Population sizes, for instance, can increase or decrease by one through wide and continuous ranges. But there is a second type of variable in homeostatic systems that should also be considered, a type which I believe to be exemplified by Tsembaga ritual. I believe that rituals among the Tsembaga may be likened to the thermostat in a heating system. In the heating system there are two variables, the values of which change through continuous ranges. One is the temperature of the medium; the other is the amount of heat flowing from a source, such as a furnace. Between them lies the thermostat, which is essentially a switch. Unlike the other two variables, this switching device has only two possible states, on and off. It is a binary variable. Ritual also has such a binary characteristic; it is occurring or not occurring, on or off. This binary characteristic is extremely important in the case of Tsembaga ritual. When the values of certain range variables reach critical values—values which endanger the system (either population or ecosystem) as a whole—a ritual is triggered; this has the effect of returning the range variables to safer levels; for instance, the size of the pig herd, the amount of land under cultivation, and the energy output of women. I do not suggest that in systems which include culturally behaving organisms, only rituals fulfil the function outlined here. I do suggest that in ecological analyses we should not confine ourselves to the description of material relations *per se*, but that we should also attempt to search for the means by which these relations are regulated; I further suggest that the regulating mechansims are likely to include components that act as switches.

HELM: In this light we can think of all structural features as mechanisms of regulation. If certain societies adopt one kind of mechanism, they may require another type in accompaniment. A certain type of residence pattern may tend to promote a certain conception of kinship, that is, a reorientation or promotion of a unilineal kinship principle. We could have a focus of association if we interpret these mechanisms of regulation as essentially meaning, in a broad sense, a standard of living in any moment of time. This involves a basic biology plus those refinements of physical life that man seeks.

RAPPAPORT: I was using the term 'regulation' in a slightly different way. I was using it to refer to operations in which system-endangering changes in the values of variables themselves initiate processes that return their values to safer levels. In other words, I was using the term to refer to processes involving 'negative feed-back.'

Several switches might be operating at once to maintain a single range variable at tolerable levels. Many self-regulating systems found in nature are redundant. Systems involving cultural components will often be highly redundant.

HELM: The concept of homeostasis may not be too appropriate. In homeostasis there are a number of homeostats that work either way to achieve equilibrium. In human existence, some desires have stronger effects than others; for example, it could be said that "it is worth cooperating to get more fish." The result of such motivation might be a change in the system.

LEE: Applying the model to the Dogrib data, we would find, for instance, that while a group occupies a fishing camp, the number of fish caught would decrease steadily and the intragroup irritations would probably increase. Then, if a family leaves because of this conflict, the fish : man ratio would become more favourable.

ROGERS: These tensions, which are the chief triggers in this mechanism, pose problems for field research. How do we assess a qualitative feature like this? Do we count the number of fights?

CARSTENS: May I give an example of an 'experimental,' controlled situation in a South African coloured reserve that I know. This reserve has a population of 4,000, and records show that forty years ago every month there were between fifteen and twenty disputes over land. However, when the diamond mines and copper mines expanded and large numbers of migratory labourers moved out to work them, the pressure on land was reduced, and there are now very few reports of land disputes.

LEE: I think that the research possibilities for this problem are very promising in the Bushmen region. I see now that group moves and sharing are fulcra, first of social relations and second of ecological relations. As food decreases, conflict increases and groups split up; then the land : man ratio becomes more favourable, or at least does not deteriorate so rapidly. Disruption of sharing may be the thermostat, in Rappaport's terms, that keeps population spaced relative to resources.

BICCHIERI: I see the manner in which people are dispersed or arranged within a boundary to be intermediary between necessities imposed by exploitative relationships and by tensions of community relationships, for example, friction. The BaMbuti have two sets of exploitative activities. The dispersion of the archer groups is accomplished easily; there is no loss in productiveness. In the net-hunting situation, additional personnel must be introduced to contribute to the efficiency of the exploitative technique. Thus the dispersal of the population is a compromise between efficiency of production and the psychic relationships of unity and disunity between the people. Other systems are intermediate in their importance as motivation for dispersal or aggregation. An example of such a process is the Dogrib hand game. There are probably a number of categories among these other systems.

CARSTENS: At times there does not seem to be this sort of association between exploitative efficiency and tensions. For instance, among the Okanagan of British Columbia, food production may increase, but it does not rise evenly for every member of the population so that the consequent unequal distribution of wealth arouses jealousies that result in conflicts or even fighting.

LEE: Bahr found that the reason the Papago took up agriculture to the extent they did (10 to 20 per cent of the food base) was prestige. It was one means by which a man could produce something beyond subsistence requirements and gain allies and influence. In this case there was a prior model for prestige seeking, and the introduced mechanism of agriculture provided its realization.

Culture and Ecology

NESBITT: I doubt that we know enough about the function of any system of culturally ritualized norms of behaviour to give a rational answer to the perfectly rational question of the value of particular customs and how they are shaped, moulded, or modified by the environment. The social organization of any culture is a complicated system of universal interaction between a great many divergent traditional norms of behaviour. To what degree may this be influenced by environment? This we cannot predict without a very thorough analysis of ecological systems; an analysis so complete in detail that we could measure what repercussion the eliminating of even one single part would have for the functioning of the whole.

In general, anthropologists have a very limited knowledge of microenvironments, and this places very definite limitations in ecological comparisons aimed at uncovering cultural correlates (for example, the necessity, often encountered in climatology, of assuming climatic conditions in areas having few, if any, stations). Such important elements as dew point, radiation, and visibility, which would prove valuable indicators of climatic analogy, are either limited or unavailable. The method of recording temperatures varies from country to country, as do also the hours of observation.

HELM: I wonder if it would be fruitful to write an ethnographic report considering the following aspects of an ecological population: (1) The dimensions of work input–output and man hours; (2) the population's distinctive means of exploitation and sharing of resources, in terms of density; and (3) its spacing of numbers and the number of spaced units within the territory. We could also add the cultural component or directives to organize chapters along these criteria or variables. I think of these variables as focal points for approaching structural features of social organization or ritual relations. If all these variables were handled in terms of an ecological population, which was making use of resources in space, and were used as a perspective from which to interpret culture, we might thereby be able to break away from the approach that starts with culture itself by looking at basic requirements that are 'precultural.'

RAPPAPORT: I would agree. Culture is commensurable, in so far as ecological relations are concerned, with the *behaviour* of populations of non-human animals.

HELM: I would like to see culture conceived of as a mechanism of a particular order of animal, and the cultures concerned as mechanisms of those entities which we may operationally define as different species.

RAPPAPORT: A culture can be regarded, so far as ecological studies are concerned, as an important part of the distinctive means employed by a human population in maintaining its relations with the other populations present in the ecosystem.

HELM: We must consider the creative aspects of culture, for example one is: culture increases population density. Not only does the population create culture but culture recreates or transforms the population.

RAPPAPORT: In precisely the same way as the lion population would increase if lions had better claws, the human population would increase if it had a better technology.

HELM: And indeed, the lion population may have to make a series of genetic adjustments by other mechanisms, in order to keep population size in equilibrium.

BICCHIERI: Culture actually redefines subsistence.

LEE: What begins to emerge from this discussion seems to be a far cry from conventional definition of culture, rather it is 'man-in-space-in-energy-exchange.' That is the static dimension. The dynamic dimension is 'man-in-space-in-energy-exchange adjusting population to resources and technology.'

BICCHIERI: I want to add that for the human population there are not only problems of subsistence but also certain conditions that are inherent in human aggregation—the community and the friction element, for instance.

LEE: Wynne-Edwards would respond that we could focus on such an element, but we must redefine our universe of investigation to "what are the implications of behaviour which cause friction (or any other behaviour) for population density?"

We need not impute causal primacy to any element in the system. We simply try to state the implications that conflict may have for density or density for conflict. We derive a tentative system composed of a number of variables on the basis of hypotheses we entertain, and we seek to determine what the relations among them are. There are no *a priori* rules concerning what we may or may not include: environmental, biological, sociological, cultural, and even such psychological variables as hostility and aggression may be included in our system. It is well to note here that the question of 'levels of integration' (inorganic, organic, superorganic) is irrelevant to this sort of analysis.

HELM: In this regard we can attempt to interpret behaviour in terms of energies and, if possible, to include the psychic energies as well. This would provide a perspective from which to view culture. Given enough food, expenditure of energy can range from subsistence to the building of skyscrapers and atomic bombs. There is a minimal point beyond which there is a continuum of energy control in terms of what man as a biological organism must expend vis-à-vis what his culture gives him to work with.

BICCHIERI: I believe that with respect to human populations (in spite of the fact that we are now treating them as ecological populations), there are important differences from animal populations that must be taken into account. Providing food is not the only problem of a population; the population has other needs.

HELM: One way to approach these other conditions is to try to gain an understanding of why man will expend the fantastic emotional and physical energy in activities that have nothing to do with subsistence.

RAPPAPORT: We start with a human population having many needs and making demands beyond those of food and shelter. But these needs, or demands, can be referred to the population as a unit, despite the fact that they are in excess of subsistence and can be treated as variables. We are, I think, working toward a single analytic framework which can comfortably accommodate kinds of events, processes, and structures that are generally handled quite separately by anthropologists.

BICCHIERI: I would separate ritual from some of the other variables, as, for instance, friction (as it is instrumental in the fission of bands), because in certain situations ritual might be a mechanism devoted to economics, while in other situations it might not. Ritual is binary, and many of the others are not.

RAPPAPORT: If I understand you correctly, you are saying that the variables that define the well-being or survival of the population should be the solid biological variables, such as caloric intake, protein intake, birth and death rates, population densities, and such environmental variables as may give empirical meaning to such terms as 'carrying capacity' and 'environmental degradation.'

SOME SUGGESTIONS CONCERNING CONCEPT AND METHOD IN ECOLOGICAL ANTHROPOLOGY

BY

ROY A. RAPPAPORT

In the preceding discussion, I suggested, as A. P. Vayda and I (1967) have elsewhere, that ecological anthropology would do well to follow leads indicated by developments in animal ecology, and I presented some further recommendations concerning these leads, some of which I shall follow up here.

I.

There is, first, the question of analytic units. These include minimally the local, or ecological, population; the ecosystem; and the regional population.

The local, or ecological, population is defined by precisely those criteria by which the animal ecologist distinguishes a population of any animal species. It is an aggregate of organisms having in common a set of distinctive means by which they maintain a common set of trophic relations within the ecosystem in which they exist together. In food-collecting and simple food-producing societies, local or ecological populations are likely to correspond to local communities. These are often territorial groups, that is, groups which claim more or less exclusive rights to exploit the resources (enter into trophic exchanges with plants and animals) occurring within demarcated areas.

An ecosystem may be defined as a system of material exchanges among the populations of the several species and the non-living substances occurring within a demarcated area. Since material exchanges rarely, if ever, discontinue abruptly at any border, it is usually necessary for animal ecologists to demarcate ecosystem boundaries, more or less arbitrarily, with reference to the research problem at hand. I suggest that it will often be convenient, and will seldom violate the facts, for the anthropologist to regard the territory of the local group as an ecosystem.

In qualification of these suggestions I might reiterate a point made in the preceding discussion. In some instances it may be illuminating to regard the territory of the local group as encompassing two or more fairly distinct ecosystems. Various complications may be encountered. For instance, not all local communities are strictly territorial; that is, against other groups similar to themselves they do not all claim the exclusive right to enter into material exchanges with populations of other animals and plants existing within a demarcated area. In such instances it may be necessary to designate a more inclusive group as the local or ecological population and a wider area as an 'immediate environment,' consisting of one or more ecosystems. In other instances we may note that two populations with very different subsistence techniques (for instance, herders and agriculturalists) may occupy and exploit a single demarcated area in different ways. We may in such instances regard the two groups as separate and distinct populations participating in a single ecosystem. Ecological populations are not discriminated in terms of the taxonomic characteristics of their memberships, but in terms of the positions, or niches, which they occupy in ecosystems; herders and horticulturalists occupying the same territory are, in reference to their ecological relations, as distinct from each other as two species of animals.

Regardless of these and other complications that may affect our discrimination of units in the field, we may analyse the relations of local or ecological populations of humans with populations occupying other niches in their 'immediate environment' in terms which do not differ fundamentally from those employed by animal ecologists.

The strategy outlined so far has certain advantages. First, the use of a general conceptual framework, in this instance one which is applicable to species other than man, is to be preferred, by the criterion of parsimony, to a special conceptual framework applicable only to man. Second, as Vayda and I have noted, the human aggregate which is distinguished, the local or ecological population, is commensurable with other units (populations occupying other niches in the ecosystem) with which it interacts to form food webs, biotic communities, and ecosystems.

These interactions are amenable to measurement and can thus be described in quantitative terms. No such advantage obtains for ecological anthropology in the more general practice of designating *cultures*, rather than *populations*, as the referent units. Cultures may induce people to polish their fingernails, but food supplies do not limit them, disease does not debilitate them, nor do predators feed upon them. It should be made clear, however, that no abandonment of the concept of culture is suggested. In the approach advocated here, the culture, or part of the culture, of a human population is regarded as part of the distinctive means by which the population maintains itself in the ecosystem.

Another advantage of the strategy being suggested here is that the units discriminated—the local population and the 'immediate environment' consisting of one or more ecosystems—are likely to be, in food-gathering and simple food-producing societies, sufficiently small in scale to be convenient units of analysis. Since this is the case, the problem of collecting sufficient data to serve as adequate samples of the interactions of the human population with the other components of the ecosystem or immediate environment, becomes manageable.

Thus far, I have made suggestions concerning only the analysis of the relations of local populations with populations occupying other niches in the ecosystems in which they participate. However, among the significant components of the environment of any local population will usually be other similar local populations occupying other areas. The local population is likely to exchange materials, usually personnel, genetic materials, and trade goods, with these other groups, and some of the processes that redistribute land among people and people over land are likely to involve two or more local populations. However, as I have suggested elsewhere (Rappaport 1967), the concept of the ecosystem as fundamentally a system of trophic exchanges between populations occupying different ecological niches within a bounded area accommodates only awkwardly non-trophic material exchanges among populations occupying similar ecological niches in separate localities. I would suggest, therefore, that, instead of diluting the concept of the ecosystem by extending it to include relations between local populations of humans, we recognize that local populations are likely to participate in, and form component parts of, a second system. This system is composed of all the local populations that occupy a geographical area and may be denominated the 'regional population.' The 'regional population' may resemble what the geneticists mean by 'population,' that is, an aggregate of interbreeding organisms capable of persisting through an indefinite number of generations in isolation from similar aggregates of the same species. In some instances the term may also be coterminous with those units that anthropologists, sociologists, and ethologists would denominate 'societies,' aggregates of organisms among which interactions take place in accordance with common sets of conventions. I might note that the concept of the regional population shares the difficulty of boundary demarcation with the concepts of the society and the breeding population. The notion of clines may be useful in social and ecological as well as physical anthropology.

Since one unit, the local population, is likely to participate in two systems, the local ecological system and the regional population system, these two systems are obviously not independent of each other. They may, rather, be regarded as more or less distinct subsystems of the larger one they together comprise, and one of the important tasks in ecological anthropology is to discover the ways in which events in the one affect events in the other.

II.

Another distinction, cross-cutting the distinction between the 'immediate' and 'non-immediate' environments (i.e., between the local ecological system and the regional population system), may be proposed. This is the distinction between what may be termed the 'operational' and the 'cognized' environments. The term 'operational environment' refers to the anthropologist's or ecologist's model of the material environment of the group under study. The term 'cognized environment' refers to the model of their environment held by a people themselves and may include components, such as spirits and gods, which cannot be shown to have empirical existence.

While it is possible to describe the material relations of a local population without reference to the views of the world entertained by its members, as Vayda and I (1967) have observed, it is reasonable to regard a people's understandings as part of the mechanism that induces individuals to behave as they do. Therefore a description of the 'cognized environment' of the group under study (if it is possible to obtain it) at least enriches our understanding of its material relations.

III.

I have been mainly concerned so far with the definition of units, but these units—the local population, the ecosystem, and the regional population—are in fact defined by material exchanges. The mode of analysis suggested by these material exchanges, as already implied in the course of this outline, is systemic. A system may be defined as a set of components related to each other in such a way that a change in the state of one produces a change in the state of at least one other. From the phenomena under observation we may abstract a number of variables and assign them values by measurement procedures undertaken in the field. The maintenance of some of these variables (e.g., land under cultivation, population size, calorie intake, energy expenditure) within certain limits, which may also be prescribed by field observation (albeit, perhaps, only approximately), may define for us the 'adequate functioning' or 'survival' of the system or systems (local population, ecosystem, regional population) under study.

Our analyses should not be confined to descriptions of material relations alone. We should also attempt to elucidate the ways in which these material relations are regulated. To put this in systemic terms, we should direct our efforts toward discovering the mechanisms by which the states of those variables that define the 'adequate functioning' or 'survival' of the systems under study are kept within viable ranges. Of particular interest in this respect are the possible homeostatic or self-regulating mechanisms 'built into' the systems with which we are concerned. A system may be said to be self-regulating when a system-endangering change in the state of a variable itself initiates activity that returns its state to a safer level, through the process known as 'negative feed-back.' In ecological systems that include man as a dominant species, I would expect the mechanisms of self-regulation to include important cultural components. Maring ritual is an example. In response to system-endangering changes in the states of certain range variables, rituals are triggered. These rituals have the effect of returning the states of the range variables to safer levels. That such ecological control functions may inhere in cultural phenomena further suggests that the place of the 'cognized environment' (the actor's model of his environment) in an analysis of the material relations of a population may be analogous to the 'memory' of the computer control in an automated system of material exchanges and transformations. In the automated system, signals concerning the states of variables are received in the memory, where they are compared to 'reference' or 'ideal' values. In response to discrepancies between actual and reference values, programs which tend to return the state of the variable to the reference value are initiated. It is reasonable to believe that similar comparisons take place between the states of components of the environments of human populations and the peoples' notions of what the states of these components should be (ideal or reference values) and that in response to discrepancies corrective programs are initiated. An important question then becomes the extent to which these reference or ideal values correspond to the actual material requirements of the local population, the ecosystem, or the regional population. In other words, we may be able to inquire into the adaptiveness of ideology in a reasonably precise way.

IV.

I think it should be clear that no abandonment of cultural anthropology's concern with the explanation of cultural phenomena is being advocated. My suggestions are concerned with making ecological anthropology more operational by placing it within the framework of general ecology, and I believe that this strategy will enhance rather than obscure our understanding of culture.

Ecological studies presently occupy a strategic position in anthropology. For one thing they offer the possibility of a new integration of cultural and physical anthropology, and they demand that bridges be built to related disciplines in the natural and social sciences. They may also bring about new syntheses within cultural anthropology. In recent years anthropologists have diverged more and more in their interests, some concerning themselves with new ways to illuminate the ideological aspects of cultures, others confining themselves more and more strictly to the study of man's observable behaviour and material relations. Ecological problems might provide an area of study in which these divergent trends can be integrated.

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